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OF

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ERRATA.

Page 23, line 18, *for* Tincture and, *read* Tincture of.
168, 11, *for* elasticity, *read* velocity.
40, *for* 277,93 tons, *read* 1243,7 tons.
41, *for* 271,15 feet, *read* 573,5 feet.
297, 29, *for* purchases, *read* purchasers.
298, 22, *for* Falcoa, *read* Falco.
300, 48, *for* Montano, *read* Montana.
301, 34, *for* Ortherea, *read* Ætherea.
303, 19, *for* Blougios, *read* Blongios.
47, *for* Fero, *read* Fern.
304, 3, *for* Islandicas, *read* Islandicus.
14, *for* unexplained, *read* unexplored.
46, *for* Fascus, *read* Fuscus.

ANNALS

OF

PHILOSOPHY.

JULY, 1822.

Article I.

On the Fundamental State of the Magnetic Phenomena of the Electrical Connecting Wire, or on the Transverse Electrical Charge. By M. Prechtel, Director of the Polytechnic Institution in Vienna. (Communicated by the Author.)

Magnetism produced by electricity is of the same nature as common magnetism; the apparently anomalous phenomena of electric magnetism may, therefore, be recognized in the phenomena of the magnetism elicited by the earth's action, or by common magnetism; and these phenomena ought to include the explanation of the phenomena of electro-magnetism. Setting out from this principle, I have made experimental researches on *transverse magnetization*, the fundamental phenomena of which were previously unknown. I believe that these phenomena give a satisfactory explanation of the physical state of the electro-magnetic connecting wire, and of electro-magnetic facts in general. I have discovered the following facts, which I have detailed in several memoirs, inserted in the first, fourth, and sixth numbers of M. Gilbert's Annales de Physique for the year 1821. These facts I shall now detail in succession.

1. When a straight iron wire has one of its ends presented to the magnetic pole, it is well known to be magnetized, or its two ends form magnetic poles of a certain degree of intensity. All circumstances being equal, this polarisation is more intense in a perfectly straight wire than in one which has angles and inequalities.

2. When an iron wire, which has its ends united accurately by welding, is magnetized in a mode presently to be described,

an endless magnet is formed, which possesses separate poles throughout its circumference, or heteronomous poles alternately succeeding each other. For these experiments the softest iron wire should be employed.

3. When the most perfect circular form is given to an endless wire, and it is suspended vertically, it will be found when examined by means of a very small magnet,* that the lower part has acquired a north pole, and the upper a south pole. By applying the pole of a magnet for some time to any part of this circular wire, it will be found that this ring is so magnetized, that its periphery presents two heteronomous poles diametrically opposite, as may be seen by Pl. XIII. fig. 1. *i i* are points of indifference. It sometimes happens that the heteronomous poles are placed from 90° to 90°, as in fig. 2; then the points of indifference are *i i i i*.

4. When an endless wire is bent in a quadrangular form, as in fig. 3, and it is magnetized by applying the heteronomous poles of a magnet to the angles *a* and *b*; then the four angles are magnetized in such manner that the heteronomous poles succeed each other alternately, as is shown by fig. 3. If the magnet is sufficiently strong, and the iron wire very soft and even, then this magnetic arrangement will take place by the application of a single magnetic pole to one angle, for example, to the angle *b*.

5. When an endless wire is bent in the form of an octagon, and we proceed as before, i. e. by applying the heteronomous poles of a magnet to the two angles *a*, *b*, fig. 4; the magnetic poles assume a similar arrangement, i. e. the heteronomous poles placed at the angles succeed each other alternately, or each north pole is followed by a south pole, or *vice versâ*. This will take place in every polygon. If these magnetic arrangements be represented as in the figure, by arranging magnetic needles, one half of these needles will be directed to the right, and the other half to the left. These facts prove that magnetic polarity has a tendency to establish itself in a right line; and it is seen that in the endless polygonal magnet a simple magnetic impulse, upon a single point of its periphery, produces a quantity of heteronomous poles, which succeed each other alternately upon this periphery.

6. This arrangement of the needles indicates the *elementary action* of each side of the polygon; this side representing a linear magnet. Nevertheless this elementary action can be observed only when the sides of the polygon possess sensible length, so that a very small magnetic needle can follow the elementary or separate action of the side, or of this linear magnet. Let us suppose that these linear magnets, forming the sides of the polygon, are extremely small (1), which will happen when the diameter of the polygon is extremely small, or the number of its

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* I find that small magnetic needles from half to one-eighth of an inch long, are extremely delicate in cases of small quantities of magnetism, even when the heteronomous poles are very near each other.

1.

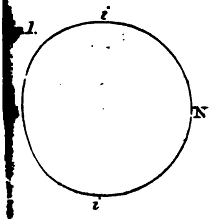


Fig. 2.

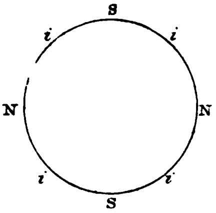
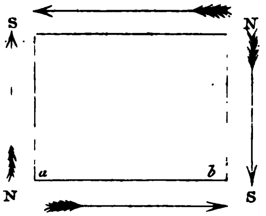


Fig. 3.



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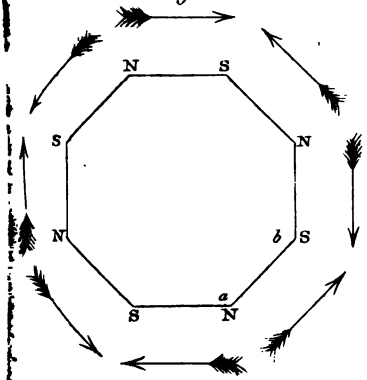

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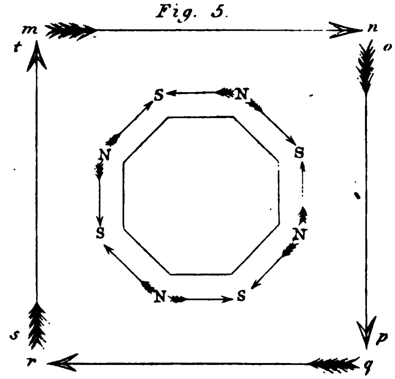

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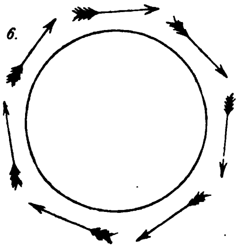

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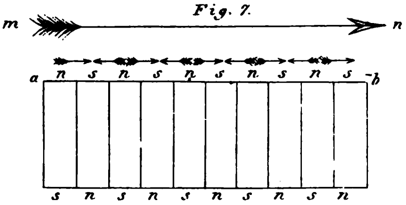


Fig. 8. P. 18.

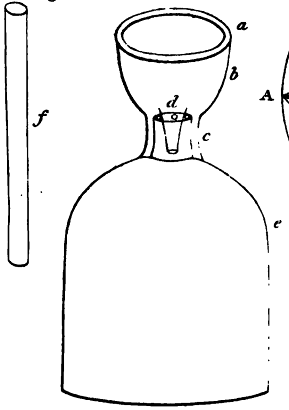


Fig. 9. P. 21.

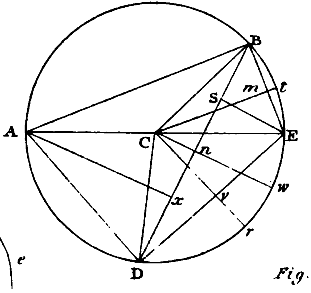


Fig. 11. P 17

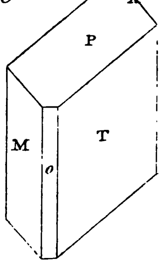
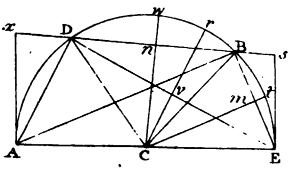


Fig. 10.



sides is very great, or which comes to the same, that the polygon of a given diameter becomes a circle; or (2) that the length of the magnetic needle employed to examine the polarity of the endless magnet is very great compared with the length of a side of the polygon; then this elementary action of each side of the polygon cannot be observed; but the combined action of all the polarizations distributed on all the sides above the diameter parallel to the needle, will take place upon the magnet. By this combined action, the needle shows an *apparent arrangement* of the polarizations in the endless magnet. This happens, precisely in the same manner, in arranging a series of magnets in the manner represented by fig. 7. These magnets are disposed one after the other in such manner, that the heteronomous poles touch and follow each other alternately in the length *a b*. In examining the magnetic arrangement *a b*, by means of a very small magnetic needle, it will be observed that the elementary actions are represented in the figure by small needles. But in employing a magnetic needle which equals or exceeds the length of the line *a b*; this needle attracted by the combined action of all the polarities, and determined by the quality of the two extreme poles of this magnetic arrangement, will assume a constant direction. This arrangement indicated by the needle *m n* is only apparent, and we should deceive ourselves if we were to conclude from its position, that the arrangement *a b* is a common magnet, presenting its poles at the two extremities, and the point of indifference in the middle. Let us suppose that the line *a b* is extremely short, it will then be impossible to examine the partial actions, and we must be content with observing the total or apparent action.

7. It is nevertheless easy to observe what happens in a polygonal endless magnet with respect to the arrangement of the needle around its periphery, if the length of the sides of the polygon are very small when compared with the length of the examining needle. In fig. 5, the needle *m n* is attracted by the poles N S N S = N S; the needle *o p* by the poles N S N S = N S; the needle *q r* by the poles N S N S = N S. The needle, therefore, preserves its direction constantly the same, around the periphery of the polygon; this would happen precisely the same upon all the points of a polygon of an infinite number of sides, or in the circle, as in fig. 6, so that there will be an appearance of the needle being directed by a current around the periphery in the same direction.

8. A superposition, or continuation of endless magnets, constitutes the *transverse magnet*, i. e. in a transverse magnet every section perpendicular to its axis is an endless magnet. The transverse magnet presents no poles at its extremities; but the heteronomous poles succeed each other alternately in the periphery of these sections. I have shown in a memoir which is inserted in the Annales de Physique already mentioned, that in forming a

helix of iron wire, and magnetizing this hollow cylinder by means of the pole of a magnet, in the direction of its axis, this cylinder becomes a transverse magnet, one side of which presents the north pole, and the opposite side the south pole. This phenomenon depends upon the fact already explained (3). I have shown that a solid bar of iron may be magnetized in the same manner by carrying homonomous magnetic poles on opposite sides in the plane of its axis. By thus treating a quadrangular bar of iron, for example, its four corners will present throughout their whole length the magnetic arrangement already explained with respect to the quadrangular endless magnet; that is to say, one corner will present throughout its whole length the north pole, the following the south pole, the third the north pole, and the fourth the south pole; and the two extremities do not exhibit signs of reciprocal polarity. By the same process, I so formed the magnetic arrangements in a cylindrical steel bar, that in one-half of its length it presented longitudinal magnetism, and in the other half, transverse magnetism. One of its ends has a north pole, and the northern magnetism diminishes to the centre, where there is indifference; here, transverse magnetism distributed throughout the periphery of the other half commences. In this arrangement, these two magnetisms support each other reciprocally.

Let us apply these magnetic phenomena to the electrical connecting wire; we shall there recognize all the properties which belong to transverse magnetism. If the connecting wire is prismatic, e. g. quadrangular or hexangular, we shall then find precisely the same magnetic arrangements as in the transverse magnets of the same form. If the wire has a cylindrical form, the magnetic arrangement is such as it ought to be according to the properties of transverse magnetism; nevertheless this arrangement is apparent, and consequently the cylindrical wire complicates the phenomena instead of explaining them, as M. Berzelius has already observed. These properties of transverse magnetism, founded on the facts now sketched, explain all the phenomena observed in the connecting wire, not only without difficulty, and without having recourse to any hypotheses of electric currents, or of certain qualities of these currents; we may also predict what will happen by varying the combinations of the experiments to which the connecting wire may be subjected. (See the memoir in the fourth number of Gilbert's Annales, 1821.)

The difference which exists between common transverse magnets and the transverse magnetic charge of the connecting wire, depends upon the nature of the action of the electric pile. Neither this pile, nor any electric force whatever, gives a simple and determinate impulse, as is the case with the action of the magnet, or with a plate of glass electrically charged (leyden jar); but the pile produces and receives these impulses every instant, so that the effect of this action is evident, notwithstanding that every instant a neutralization of the electricity, or destruction of the electric effect occurs. To this property must be attributed the reason why the pile magnetizes some metals, which are not magnetized to a sensible degree by the magnet; for although these metals have not the power of retaining the magnetic charge, and suffer more or less every instant the neutralization of the communicated electricities; yet as the pile establishes this electric tension each moment, it is impossible that these metals should not exhibit its effect; that is to say, the transverse or magnetic charge. The case is the same with the electric spark, because the electric spark is nothing else than *a connecting wire formed by the air*, as is proved by the experiments of Sir H. Davy, by the magnetic property of the electric spark.

This activity of the pile prevents, at the same time, the fixed arrangement of the poles in the periphery of the connecting wire, as produced by common magnetization. If, for example, the connecting wire has any kind of prismatic form, the pole of angle S *i*, fig. 5, which was south, becomes north, when the bar is turned, until the angle S *i* occupies the place of the angle N 2; and the angle N *i*, which was north, becomes south, when it enters the place of the angle S *i*.*

According to this, the researches into the phenomena of the electrical connecting wire resolve into this simple question: Why is a conducting body connecting the two electrical poles charged transversely? The answer to this question enters into the theory of electricity in general, and I shall probably find an opportunity of returning to it. The properties of the *transverse electrical charge* form a new branch in the theory of electricity. The facts stated readily lead to the result that *every transverse electrical charge is magnetic*. The reason is, that in the transverse electrical charge, the electrical poles approach each other infinitely near, and on account of this approximation, their tension is increased. Let us suppose that two weak electrical poles of the intensity = *i* are capable of giving the longitudinal electrical charge to a metallic wire of 100 feet long, and the thousandth of an inch in thickness, and that this longitudinal charge becomes changed into a transverse electrical charge, then the intensity of the poles at the periphery will be greater than 1200000 *i*. As two very weak electrical poles are capable of giving an electrical charge to a much longer metallic wire, it follows that the electrical tension of the poles in the connecting wire must be very great, in comparison with common electrical tensions, for which the air still preserves its non-conducting power, although it is diminished on account of this tension. It is this great electrical tension of the transverse charge, which makes the metals red-hot, and volatilizes them. This constant and infinitely great electrical tension is magnetic; for electricity whose tension is so considerable, that to it all non-conductors become conductors, and some bodies only which were before conductors, become on account of the state of their internal cohesion non-conductors, can only be magnetism. This I have shown in a memoir in the first number of Gilbert's Annales for 1821. If the air were not a non-conductor of electricity, we should not be acquainted with common electricity, but only with magnetism.

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* These references are not in the figure, but being apprehensive that I might alter the sense, I have not attempted to supply the deficiency.—*Ed.*

Article II.

Meteorological Account of the Weather during the Three Winter Months of the Years 1821 *and* 1822, *kept at Jasmond, Newcastle-upon-Tyne. With Observations on the Time of the Flowering of various Plants.* By N. J. Winch, Esq.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Newcastle-upon-Tyne, April* 8, 1822.

Conceiving a meteorological account of the weather during the three winter months of the years 1821 and 1822 as it occurred in the north-east of England, may afford amusement to some of your numerous readers, I take the liberty of transmitting an abstract of a journal kept at Jasmond in the vicinity of this town, by Mr. Losh, and kindly communicated by him for that purpose. Together with observations on the state of the weather, notes on the time of the flowering of various plants are interspersed, which, with some general remarks, will serve to convey a correct idea of the mildest winter experienced in this part of the island within the memory of man.

I remain, Sir, your obedient servant,

N. J. Winch.

Jasmond, one mile north of Newcastle-upon-Tyne, about 200 feet above the level of the river Tyne. Lat. 55° N.

1821.—Nov. 30. This has in general been a mild and pleasant month, favourable to vegetation, and to all kind of farming operations. There were, however, during it, two or three very heavy storms of wind, one in particular in the night of the 30th, perhaps as violent for two or three hours as any within the memory of man. As, however, the wind blew from the W., no great injury has been done by it to the shipping on the coast.

Plants in Flower.—China rose, Portland thorn heath (*Erica carnea*), jasmine mignonette, purple groundsel, stocks, panzey hollyhocks, wall flowers, carnation, colchicum, gentianella, viola, auricula, primula, Canterbury bell.

Month.	Hour.	Weather.	Wind.	Ther.	Barom.
1821.					
Dec. 1	9	Clear and windy.	W	42°	29·1
	2	Ditto.	W	41	29·0
	10	Windy night.	W	37	29·4
2	9	Clear morning; hoar frost.	—	38	29·5
	2	Clear and sunny.	W	42	29·5
	10	Calm and clear.	—	40	29·6
3	9	Gloomy morning; wet night.	—	40	29·4
	2	Clear and fine.	W	38	29·4
	10	Clear and pleasant night.	—	35	29·5
4	9	Showery morning.	NE	40	29·4
	2	Cloudy, with showers.	NE	44	29·3
	10	Calm and clear night.	NE	39	29·4
5	9	Showery morning.	—	41	29·3½
	2	Showers.	W	39	29·3
	10	Clear windy night.	—	40	29·5½
6	9	Frosty night; clear morning.	—	33	30·1
	2	Clear and sunny.	W	36	30·1½
	10	Calm; clear and frosty.	—	33	30·1
7	9	Gloomy and windy.	S	37	29·8
	2	Stormy day.	S	38	29·5
	10	Clear and windy.	W	43	29·4
8	9	Pleasant morning.	—	44	29·6½
	2	Cloudy, but dry.	SW	48	29·7½
	10	Calm and mild rain.	—	51	29·7
9	9	Calm and mild.	—	52	29·6
	2	Calm and warm.	SW	53	29·6
	10	Calm and cloudy.	—	51	29·6
10	9	Hazy morning.	—	49	29·6½
	2	Cloudy, with showers.	S	52	29·6
	10	Calm and cloudy.	—	52	29·6
11	9	Clear and sunny.	—	38	29·8
	2	Ditto.	W	42	29·9
	10	Calm and clear.	—	36	29·9
12	9	Cloudy morning.	SW	37	29·7
	2	Windy, with showers.	SW	41	29·7
	10	Very windy; clear.	SE	45	29·8
13	9	Calm, and very gloomy.	—	45	29·6½
	2	Ditto.	SE	45	29·6
	10	Calm, mild, and clear.	—	43	29·6½
14	9	Calm and hazy; hoar frost.	—	44	29·7
	2	Gloomy; showery.	SE	44	29·6½
	10	Calm and fair.	—	45	29·6
15	9	Calm and hazy; small rain.	SE	48	29·6½
	2	Gloomy, with showers.	SE	48	29·6
	10	Clear; windy.	SW	47	29·5
16	9	Pleasant morning.	—	—	—
	2	Very mild.	—	—	—
	10	Dry and hazy.	—	—	—
		During the two last days, the bees have come out in great numbers, and flies and moths have been frequent.			
17	9	Calm and pleasant showers.	—	48	29·1½
	2	Very fine.	SE	47	29·1
	10	Gloomy.	—	45	28·9½
18	9	Calm and pleasant.	S	47	28·6
	2	Very fine.	SW	46	28·6
	10	Clear and pleasant.	SW	43	28·7
19	9	Calm and pleasant.	—	38	28·7
	2	Cloudy, with showers.	SW	43	28·7
	10	Calm and clear.		37	29·0

Month.	Hour.	Weather.	Wind.	Ther.	Barom.
1821.					
Dec. 20	9	Calm and pleasant.	SW	38°	29·0
	2	Very fine day.	SE	42	29·0
	10	Windy, with rain.	SE	37	28·6½
21	9	Clear morning.	—	35	28·6
	2	Cloudy and gloomy.	NW	39	28·6½
	10	Calm and clear.	—	39	29·0
22	9	Hoar frost.	SW	38	29·1
	2	Cloudy, with showers.	SE	40	29·0
	10	Calm and wet.	SE	39	28·9
23	9	Gloomy, with showers.	SW	35	28·8
	2	Fine, but showers at one o'clock.	SW	36	28·7
	10	Gloomy drizzling.	W	35	28·5
24	9	Hazy morning; hoar frost.	SW	35	28·8
	2	Gloomy, with rain.	SW	36	28·7
	10	Misty; very wet.	W	35	28·5
25	9	Heavy rain.	NW	37	28·1
	2	Gloomy, with rain.	W	38	28·1
	10	Calm and star-light.	W	35	28·3
26	9	Calm and clear; frosty.	—	33	28·5
	2	Ditto.	SW	35	28·5
	10	Ditto, ditto.	—	32	28·6
27	9	Hazy morning; hoar frost.	S	33	28·7
	2	Very fine.	SE	38	28·6½
	10	Gloomy.	SE	38	28·8
28	9	Misty morning; hoar frost.	SW	36	28·8
	2	Cloudy and windy.	SW	41	28·6
	10	Wet and windy.	SE	42	28·4
29	9	Gloomy; very wet.	SE	44	28·4
	2	Stormy, with rain.	SE	44	28·4
	10	Dark and very wet.	NE	43	28·7½
30	9	Gloomy and wet.	NE	42	29·0
	2	Clear and sunny.	W	43	29·0½
	10	Windy and unsettled.	W	41	29·5
		Bees flying abroad.			
31	9	Fine and clear.	NW	37	29·7
	2	Ditto.	NW	42	29·7
	10	Clear and frosty.	W	33	29·8

Plants in Flower.—China rose; wall flowers, single and double; stocks, single and double; gentianella (gentiana acaulis); sweet pea; heath (Erica carnea), polyanthus, auricula primula; pansey; blue hepatica; carnations.

The flowers of many of the above, as the carnation, sweet pea, &c. are very feeble, but still sufficient to show the remarkable mildness of the season, or rather the remarkable want of frost. We have not as yet this winter had snow on the ground, nor any frost beyond slight occasional morning hoar frosts. I have never seen the mercury below the freezing point. We have had a good deal of heavy rain, but still in this district scarcely our average quantity; and from the long continued drought in the summer and autumn, I am inclined to think that more would be beneficial. Though, during this month, we have had no very violent wind, yet we have had frequent windy weather, and it has blown much more from the SE than usual. Wheat looks well, and, I think, all the operations of husbandry have been carried on without interruption.

Month.	Hour.	Weather.	Wind.	Ther.	Barom.
1822.					
Jan. 1	9	Gloomy morning; hoar frost; fair.	SW	34°	29·5
	2	Cloudy and wet.	SE	37	29·7
	10	Stormy night.	SE	33	29·3½
2	9	Gloomy; showers of sleet.	—	33	29·5
	2	Gloomy and calm.	NW	35	29·5
	10	Calm and cloudy.	—	34	29 6½
3	9	Calm and hazy.	SW	34	29·6
	2	Ditto and gloomy.	S	35	29·5½
	10	Ditto and cloudy.	S	33	29·4
4	9	Stormy and wet.	—	36	29·5
	2	Ditto, with snow showers.	NE	37	29·5
	10	Ditto.	—	37	29·8
5	9	Hoar frost, with showers of snow.	NW	34	29·8
	2	Stormy, with snow.	N	34	29·8
	10	Calm and frosty.	N	32	30·0
6	9	Hoar frost, with showers of snow.	NW	34	30·0
	2	Calm and clear.	SW	34	30·0
	10	Cloudy, with slight snow showers.	SW	32	29·9½
7	9	Gloomy, with sleet.	—	35	29·9½
	2	Ditto.	NW	35	29·9
	10	Calm and clear.	—	33	30·1
8	9	Gloomy.	—	35	30·0
	2	Cloudy and windy.	NW	38	30·0
	10	Gloomy.	—	40	30·0
9	9	Clear and pleasant.	—	37	30·0½
	2	Ditto and windy.	NW	38	30·0
	10	Cloudy and windy.	—	42	29·9
10	9	Cloudy.	—	44	29·8
	2	Calm and fine.	W	46	29·8
	10	Calm and pleasant.	—	41	29·8½
11	9	Windy, but pleasant.	—	46	29 0
	2	Ditto and gloomy.	W	46	29·0
	10	Ditto, but pleasant.	—	43	29·9½
12	9	Pleasant.	—	45	30 0
	2	Ditto, but windy.	W	47	30·0
	10	Windy and cloudy.	—	49	29·9
13	9	Very windy.	—	49	29 9
	2	Ditto and cloudy.	SW	47	29·8
	10	Ditto and clear.	—	46	29·8
		Yellow aconite in flower.			
14	9	Stormy, with showers.	SW	38	29·8
	2	Clear, and windy.	W	42	29·8
	10	Windy, with showers.	W	38	29·8
15	9	Very fine.	—	35	29·9
	2	Clear and windy.	W	35	29·9
	10	Clear and frosty.	—	32	29·9½
16	9	Clear and fine.	—	32	30·0
	2	Ditto.	W	33	30·0
	10	Ditto and frosty.	—	30	30·0
17	9	Calm and fine.	—	35	29·9
	2	Ditto.	W	40	29·9
	10	Ditto.	—	35	29·9
18	9	Cloudy and windy.	—	43	29·9½
	2	Ditto.	SW	43	29·9
	10	*Ditto and very windy.*	—	43	30·0

Month.	Hour.	Weather.	Wind.	Ther.	Barom.
1822.					
Jan. 19	9	Calm and pleasant.	—	45°	30·0½
	2	Very fine.	SW	45	30·0
	10	Gloomy and windy.	—	48	29·8
20	9	Clear and windy.	—	44	29·8
	2	Very fine.	W	45	29·8
	10	Calm and clear.	—	42	29·9
21	9	Clear and windy.	—	44	30·0
	2	Very fine.	W	45	30·0
	10	Calm and mild.	—	44	30·0
		Snow-drops and crocuss in flower.			
22	9	Very fine.	—	42	30·0
	2	Ditto.	SW	47	30·0
	10	Calm and pleasant.	—	44	30·0
23	9	Very fine.	—	43	29·9
	2	Ditto.	SW	46	29·8
	10	Gloomy.	—	46	29·7
		Birds, say thrushes, larks, robins, and hedge-sparrows, singing this evening as they do in April.			
24	9	Calm and pleasant.	—	46	29·6
	2	Very fine.	SW	47	29·6
	10	Gloomy.	—	45	29·6½
		We have in flower in the open air 28 distinct kinds of shrubs and plants.			
25	9	Windy and cloudy.	W	44	29·6
	2	Ditto and clear.	SW	44	29·6
	10	Windy.	W	39	29·8
26	9	Clear and windy.	—	38	29·9
	2	Ditto.	W	39	29·9
	10	Gloomy and cold.	—	35	30·1
27	9	Calm and misty.	—	36	30·1
	2	Ditto and wet.	SW	39	30·0½
	10	Dark, windy, and wet.	—	46	30·0
28	9	Clear and windy.	—	45	29·9
	2	Ditto.	SW	44	29·8
	10	Gloomy.	—	46	29·9
29	9	Clear and pleasant.	—	40	29·9½
	2	Ditto.	W	43	29·9½
	10	Calm and very clear.	—	35	30·0
30	9	Clear and hoar frost.	—	—	30·0½
	2	Ditto and fine.	W	—	30·0
	10	Gloomy and windy.	—	—	30·0
31	9	Stormy night; windy morning.	—	41	29·9
	2	Clear and high wind.	SW	43	29·8
	10	Fine night; windy.	—	42	29·8

In Flower.—China rose, laurustinus, common furze, pansey, yellow aconite, wall flower, christmas rose, stocks, hepaticas of different colours, polyanthus, auricula, primrose, gentianella, heaths, crocus, snow drops, pimpernel, dwarf rhododendron (rhododendron hirsutum), hellebore, and mespilus japonica. This month of January has been the mildest ever remembered, and certainly very favourable for gardening and planting. We have had some very heavy gales of wind, and several slight ty nights, but nothing like snow storms, which are usual his season of the year. The crops, particularly that of wheat, he last autumn, have proved very productive in the northern part of the kingdom at least, and of a good quality.

nth.	Hour.	Weather.	Wind.	Ther.	Barom.
22.					
eb. 1	9	Pleasant morning.	—	45°	29·7
	2	Clear and windy.	SW	46	29·6
	10	Very stormy night.	—	47	29·3½
2	9	Stormy, with rain.	—	48	29·1
	2	Ditto.	SW	51	29·0
	10	Ditto.	—	51	28·6
3	9	Clear morning; hoar frost.	—	35	29·1
	2	Ditto and pleasant.	SW	42	29·2
	10	Ditto and calm.	—	37	29·3
4	9	Calm and clear; hoar frost.	SW	35	29·4½
	2	Clear.	SW	40	29·4
	10	Gloomy, with showers.	SE	39	29·2
5	9	Very fine.	—	40	28·9
	2	Windy, with rain.	SW	39	28·8
	10	Ditto and clear.	—	35	29·5
6	9	Clear and pleasant; hoar frost.	—	37	29·8
	2	Clear and fine.	SW	42	29·8
	10	Gloomy and windy.	—	39	29·5½
7	9	Wet night; gloomy morning.	SE	42	29·4
	2	Cloudy and windy.	SW	47	29·4
	10	Calm and pleasant.	SW	41	29·5
8	9	Calm and hazy.	NW	41	29·5
	2	Very fine and clear.	W	42	29·5
	10	Calm and clear.	W	43	29·4½
9	9	Gloomy and windy.	—	41	29·5
	2	Cloudy.	SE	42	29·4½
	10	Calm and wet.	—	43	29·5
10	9	Gloomy; calm; fair.	—	43	29·6½
	2	Cloudy and calm.	S	44	29·5
	10	Calm and clear.	—	42	29·5
11	9	Calm and clear; hoar frost.	—	36	29·7
	2	Clear and fine.	SW	44	29·7
	10	Calm and clear.	—	36	29·7½
12	9	Clear and hoar frost.	—	34	30·0
	2	Clear and sunny.	W	42	30·0
	10	Calm and clear.	—	36	30·0
13	9	Calm and hazy; hoar frost.	—	33	29·9½
	2	Calm and cloudy.	SE	37	29·8½
	10	Calm; hazy and wet.	—	38	29·8
14	9	Pleasant day.	—	39	29·8
	2	Very fine.	SE	47	29·8
	10	Gloomy and windy.		42	29·7
15	9	Calm and pleasant.		45	29·7½
	2	Clear and sunny.	SW	40	29·8
	10	Gloomy. Mignonette in flower.	—	41	29·8
16	9	Clear and fine morning.	W	37	29·9
	2	Cloudy.	SW	42	29·9
	10	Wet evening; starlight, but windy night.	SW	49	29·8½

Month.	Hour.	Weather,	Wind.	Ther.	Barom.
1822.					
Feb. 17	9	Mild and pleasant.	SW	50°	30·0
	2	Very fine day.	W	54	29·9
	10	Gloomy night; windy.	W	48	29·9½
18	9	Windy, but pleasant.	SW	48	29·9
	2	Ditto, with showers.	W	47	29·9
	10	Calm and starlight.	W	48	30·1
19	9	Cloudy.	SW	42	30·1
	2	Ditto and windy.	W	47	30·0
	10	Ditto and calm.	W	46	29·9
20	9	Gloomy and wet.	—	44	29·5
	2	Clear and windy.	W	44	29·5
	10	Calm and clear.	—	44	29·7
21	9	Clear and sunny.	—	44	29·9
	2	Calm and sunny.	W	45	29·9
	10	Calm and cloudy,	—	42	30·1
22	9	Pleasant, but windy.	W	43	29·9
	2	Ditto.	W	45	29·9
	10	Windy, with showers.	SW	40	29·7
23	9	Clear, with hoar frost.	—	40	29·9½
	2	Clear and windy.	SW	45	29·8
	10	Windy and starlight.	—	45	29·7
24	9	Cloudy, with slight showers.	—	48	29·5½
	2	Ditto and windy.	SW	52	29·5
	10	Ditto very windy.	—	52	29·6½
25	9	Windy, but mild.	—	51	29·8
	2	Ditto and clear.	SW	54	29·7½
	10	Cloudy and windy.	—	57	29·7
26	9	Gloomy, with showers.	—	47	29·7
	2	Clear and fine.	SW	47	29·7
	10	Ditto and windy.	—	39	29·8½
27	9	Clear and sunny.	—	37	30·1½
	2	Sunny and fine.	W	44	30·2
	10	Calm and clear.	—	47	30·3
28	9	Clear and fine.	W	40	30·3
	2	Clear and sunny.	SW	45	30·3
	10	Calm and clear.	SW	37	30·2

In Flower.—Crocus, snowdrop, hepatica, aconite, violet, wall flower, heath, mezerion, anemone, stock, pansey, polyanthus, primrose, auricula, primula gentianella, dog tooth, violet, christmas rose, pimpernel, carnation. The month of February has this year been remarkable for its mildness. We have had no frost beyond occasional hoar frosts in the morning, and although there have been some violent gales of wind, and one or two heavy falls of rain; upon the whole, all the operations of husbandry and gardening have been carried on without interruption, and in the most favourable manner. At the same time, the frosty mornings have given a salutary check to vegetation, and prevented that premature expansion of leaves and flowers which too often ends in the ruin of our crops of fruit, particularly of apples and pears.

ARTICLE III.

n abstracted Statement of the Weather during the Twenty Years from 1772 to 1792, kept at the Imperial Academy of Sciences in St. Petersburgh; to which is added the Two Years 1818 and 1819; both copied from the Russian Yearly Calendar. By Mr. Longmire.

ccount of the Weather at St. Petersburgh from the Year 1772 to 1792, both included, in the Old Stile.

Barometrical Observations.

	English inches.
1. Greatest height during these 20 years occurred on Nov. 23, 1774	31·15
Least height in 1784	28·56
Difference	2·59
2. Mean of the greatest yearly height taken on the 20 years	30·841
Mean of the least height	28·813
Difference	2·028
General mean height	29·914

In general, the barometer fluctuates the most; and, therefore, at the highest and lowest points in December; but the mean ight was at the upper extreme in May, and at the lower in July.

Thermometrical Observations.

The account is given in the scales of Reaumur and De Lisle. have reduced the numbers to Fahrenheit's scale, and annexed em to the others.

	Fahrenheit.	De Lisle.	Reaumur.
The greatest cold on the 4th of Feb. 1772	−38°	208·0	−39$\frac{1}{9}$
The greatest heat on the 7th of July, 1788	92	100·0	26$\frac{2}{3}$
The difference	130	108·0	66$\frac{1}{2}$
The mean of the greatest cold for the 20 years	−23	195·6	−24$\frac{4}{9}$
The mean of the greatest heat	85	106·6	23$\frac{7}{16}$
The difference	108	89	48$\frac{1}{8}$

	Fahrenheit.	De Lisle.	Reaumur.
3. *a.* Mean temperature in the mornings and evenings	33½	149·7	—
b. The same during the summer, or from May till October	49	136·0	7½
c. The same during the winter, or from November till April	20	160·5	– 6¼
4. *a.* Mean temperature at two o'clock after mid-day	43¼	141·0	4¼
b. The same during the six summer months	61	126·0	12¼
c. The same during the six winter months	27	154·8	– 2¼

In general, January was the coldest, and July the hottest month.

The first frost was always between the 8th of Sept. and the 9th of Oct.; but in general about the 27th of Sept.; and the last frost always between the 1st of April and the 12th of May, but mostly in April.

Each year had about 112 complete winter days, 59 harvest and spring days, with frost in the night, and 194 summer days.

The ice in the Neva River at St. Petersburgh was broken up sometimes on the 22d of March, generally on the 1st of April, and never after the 31st of this month. This river was never frozen again before the 16th of October, mostly on the 14th of November, and never later than the 12th of December. The river was navigable generally 218 days, and covered with ice 147 days.

Each year had, for the most part, 69 perfectly calm days, 166 days of strong wind, 103 windy, and 27 very stormy days. The west wind prevailed the most, and the south wind the least. January was the most stormy, and had westerly winds, and July was the calmest month. The north wind reigned in April, the east in July, the south in November, and the rest in August.

Each year had 91 fair days, 118 completely dull, 156 partly cloudy days, 106 rainy, 73 showery, 43 foggy, and 4 times hail; 13 to 14 times thunder, and 21 northern lights. In the year 1786, it thundered 18 times; in 1790 only 6 times: these are the extremes in 20 years.

The year 1774 had the most thunder and northern lights, the thunder having been heard 17 times, and the northern lights seen 48 times. It is remarkable that the northern lights have decreased since 1782, as from that year to 1786, they were seen 110 times, and only 39 times from 1787 to 1791.

The most serene months were April and June, next to them March, May, and July; November, December, and January, were the dullest months; August was the most cloudy and variable, and next to it, the months July, May, and September. The greatest fogs are in February, and the most rain in July, August, and September; the most snow falls in December. It hails the most in May; in September, somewhat less, but never in January and February; in December, only twice in 20 years; in March and November, four times; and in June, five times.

The northern lights abounded in September and March, and July had the most thunder; the former having been seen four times each month, and the latter heard four or five times. During 20 years, it thundered three times in June and August; June and July had no northern lights, and December, January, and September, had no thunder. In November, it thundered only once; in April it occurred five times, in October three, and in November, two thunder storms.

◆

Statement of the Weather for the Years 1818 *and* 1819.

Barometrical Observations.

	1818.	1819.
	Eng. in.	Eng. in.
1. *a.* The greatest height was in 1818 on Oct. 5, and in 1819 on Feb. 25.	30·88	31·32
The least height on Jan. 4, in 1818; and on Nov. 26, in 1819	28·28	28·86
b. The difference	2·60	2·46
The mean height	29·58	30·09
c. The mean height from three observations every day in the year	30·17	30·09
	Days.	Days.
d. The mercury stood higher than 29·86 English inches during ..	253	281

Thermometer.

	1818.	1819.
1. *a.* Greatest cold by Fahrenheit		
The greatest cold in 1818 was on Feb. 17, and in 1819, on Dec. 17, below zero	12·66	34
b. Greatest heat on July 9, and on May 27, in the respective years.	88·0	90
	100·66	124
	Days.	Days.
2. *a.* There were days in which the temperature was below the freezing point in the mornings and evenings	150	174
b. In the above days, there were, in which the temperature was		
30° below zero	—	2
30° to 18° below zero	—	3
18° to 6° ditto	4	9
6° below to 8° above zero.	6	35
8° to 20° ditto	20	41
20° to 32° ditto.	112	84
	150	174

	1818.	1819.
	Days.	Days.
3. *a.* There were days in which the temperature rose higher than the freezing point in the warmest part of the day.	273	229
b. Of these days, there were, in which the temperature was 80°.	15	15
Between 80° and 67°	38	46
67° and 55°	64	68
55° and 43°	63	48
43° and 32°	93	52
	273	229
4. *a.* The mean temperature in the mornings and evenings from the 1st of November to the 1st of the same month each year.	20°	24
b. And the mean temperature at mid-day	60	61
c. But the mean temperature, from observations three times a day, taken soon after mid-day, was	45	39
The same for the mornings and evenings	37	27
d. General mean temperature, from three observations each day	40	35
5. *a.* Last frost in the first part of the year was on May 18, 1818, and April 28, 1819.		
First frost in the latter part of the year was on Sept. 13, 1818, and Oct. 15, 1819.		
	Days.	Days.
b. Wind blew; very stormy or hurricanes	14	17
Very windy	25	76
Windy	60	218
Moderately windy	251	
Calm	15	54
c. North wind prevailed	27	
East	51	
South	34	
West	124	
North-east	36	
South-east	24	
South-west	32	
North-west	22	
d. Perfectly clear days	72	
Partly clear and partly cloudy	153	
Overcast	140	
e. There were in the above, misty days	218	
6. *a.* Rain fell during	88	68
Snow	77	89
Hail, times	27	6
b. First snow was on Oct. 14, 1818, and Oct. 17, 1819.		
Last snow was on May 5, 1818, and May 10, 1819.		
c. The water continued frozen	259	228
The summer had	118	169
	Eng. inches.	Eng. inches.
d. The rain, hail, and snow, were equal to a column of water of	12·84	16·96
The proportion of water yielded, from rain to that from snow, was as	5 to 2	4 to 3
	Times.	Times.
7. *a.* Thunder was heard	12	16
First thunder occurred on April 15, 1818, and April 6, 1819.		
Last thunder was on the Sept. 1, 1818, and Sept. 18, 1819.		

	1818.	1819.
	Times.	Times.
h. Rainbows were seen	3	7
Circular round the sun occurred	7	9
Circular round the moon	5	13
Northern lights were seen	6	12
8. a. The river Neva was frozen on Nov. 15, 1818, and Oct. 27, 1819.		
b. The ice was broken up on April 17, 1818, and April 9, 1819.		

ARTICLE IV.

On the Crystalline Form of Diaspore. By W. Phillips, FLS. &c.

(To the Editor of the *Annals of Philosophy.*)

In the *Annals of Philosophy* for the present month there is a communication on the subject of that rare mineral diaspore. In the cabinet of my friend, S. L. Kent, is another specimen considerably resembling that which is in the possession of G. B. Sowerby, but of a more highly crystalline structure.

The results obtained by subjecting the latter to the blowpipe by J. G. Children, Esq. tend to show that it does not differ essentially in character from the diaspore of Le Lievre; and having been permitted to examine the specimen in the possession of S. L. Kent, which he bought many years ago at a sale of foreign minerals, without either a name or locality, I am satisfied that it also is a true diaspore.

I succeeded in detaching portions of several fragments of crystals, and one very minute and nearly perfect crystal, sufficiently brilliant for the use of the reflective goniometer.

This crystal is a doubly oblique prism, of which the form and measurements are shown in Pl. XIII. fig. 11.

M on T	65°	0′
P on M	108	30
P on T	101	20

The plane *o*, though perfectly defined, is not brilliant enough for the use of the reflective goniometer, nor are three extremely minute planes in connexion with it at the lower solid angle of the prism.

I possess a very small fragment of Le Lievre's diaspore, on which the plane P of the preceding figure is very bright, and on which there is a cleavage parallel to the plane M, and hence I have been enabled to procure the measurement by the reflective goniometer of 61° 40′, which is so near to the complement of P on M (108° 30′) that it is impossible to doubt its being P on M *return* over the edge X. Nor can a reasonable doubt exist that the two specimens are identical, though differing somewhat in appearance. They afford similar results on the application of heat. W. Phillips.

Article V.

On the Differences in the Annual Statements of the Quantity of Rain falling in adjacent Places. By H. Boase, Esq. Treasurer of the Royal Cornwall Geological Society.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Geological Society, Penzance, June 1, 1822.*

Your intelligent correspondent Mr. Hanson, noticed (*Annals* for May, p. 372), that "the differences in the annual statements of rain from places near together are singular, and certainly require an attentive inquiry." The tables published from time to time, and periodically, in the *Annals of Philosophy*, exhibit still greater "differences" than those stated by Mr. Hanson; and it was in consequence of remarking such anomalies that the resident officers of the Cornwall Geological Society instituted, above 12 months ago, a course of careful observations, with a view to an explanation of this phenomenon.

Suspecting that a part, if not a great part, of the differences arose from the disparity of gauges or measures, our first care was to be accurate in that respect. Fig. 8 (Plate XIII), will show better than a mere verbal description, the form of the instruments we adopted. The upper rim, *a*, is of copper, one inch wide; the basin or funnel, *b*, is of pewter, two inches deep; the outer neck or cylinder, *c*, is of the same material, as is also the pipe, *d*. This cylinder should so fit the neck of the bottle or receiver, *e*, as to keep the funnel quite steady; and the pipe has a very small orifice at its lower end, and is covered by a perforated lid at top, in order to prevent, as much as possible, *evaporation*. The diameter of the copper rim is exactly six inches, and correctly turned in a lathe and perfectly circular. All this any expert brazier can accurately execute, and for the cost of 4*s.* or 5*s.* A common bottle does well for the receiver, but it should be of the capacity of not less than three pints wine measure, for a gauge of six inches diameter.

No less care is requisite in having a *measure* accurately graduated. A cylindrical glass jar or gas receiver, *f*, is very convenient, and easily obtained. In a guage of six inches diameter one inch deep of rain is equal to 28·274 cubic inches (nearly one pint wine measure), and in weight at the temperature of 60° to 7139·1850 grains; consequently ¼ inch = 1784·7962 or 1784¾ grains nearly. Let, therefore, the glass jar be exactly balanced in good scales, and then filled with *rain* or *distilled* water to the perfect equipoise of 1784¾ grains, marking the height on the side of the measure, which will be the indication of ·25 or ¼ inch of rain in the guage. Were the cylinder of equal diameter throughout, it would only remain to divide the space so marked off into 25 equal parts; but as these glasses are seldom quite uniform, it is necessary to check the measure by weight to every ·05 of an inch, which is easily done by weighing with 1427¾ grs. for ·20, with 1070¾ for ·15, with 714 for ·10, and with 357 grs. for ·05 of an inch, marking these divisions severally and accurately on the side of the measure, and then dividing each into five equal parts, observing to allow in the lowest for the bulb usually found at the bottom. Thus the measure will be graduated to the hundredths of an inch, and if it be about 1½ inch in diameter, the spaces will be large enough to halve, so that the register may be conveniently kept in three places of decimals; thus instead of ·10½, set down ·105, being so many thousandths of an inch. If there is any difficulty to cut the glass, the graduation may be marked with a pen on a slip of paper pasted on the side of the measure, observing that it should be quite dry before the operation takes place.

It is thus easy to provide an accurate pluviameter; but to find a suitable situation for fixing it "at a sufficient distance from trees, buildings, or any object that might obstruct" the free current of the wind, is a matter of great difficulty, and of the greatest importance. For this reason, the tops of the highest buildings have been heretofore selected. There is, however, cause to suspect that they are the *most ineligible.* In the reports from Kinfaun's Castle, the upper is made to indicate more rain than the lower guage. This is contrary to all similar observations, and to the nature of things. The error must, therefore, be in the different capacity or situation of the instruments used. To this phenomenon, which has excited so much speculation, our observations have been particularly directed. Three guages and measures, exactly alike in form and size, have been used. The first was already fixed on the top of our Museum (from this our annual reports are drawn), higher than the level of the adjacent chimney stacks, and consequently free from lateral obstructions. The house is open in front, joined on each side to others, and its back towards the continuous buildings of the town. The guage is 45 feet above the surface of the ground, and 143 above the level of the sea. The two other guages are fixed at the *level of the ground,* each in a garden, and at a distance of about 60 yards from any building or high trees, being in respect of all circumstances, that seem to affect the fall of rain, *similar* each *to the other.* The first of these two, or No. 2, is 150 yards distant from the Museum, and 90 feet above the sea. No. 3 is about 500 yards from both the former, and 70 feet above the level of the sea. In these two, viz. No. 2 and No. 3, the quantity of rain was on the whole equal, only varying occasionally in a small degree above or below each other; but the difference between them and No. 1 is very great, viz. above 3 to 2, the result of 12 months being, in No. 1, 30·475 inches; and in No. 2 and No. 3, 46·080 inches.

The ratio varied considerably in several months, as for instance, the total of

	No. 2 and 3.	No. 1.				
Aug. 1821, was in.	4·470	.... 4·000	being nearly as	9	to	8
September.......	4·520	.... 3·190		9		6
October.........	5·770	.... 3·550		11		7
November	5·170	.... 3·110		10		6
December	9·500	.... 6·480		19		13

These months were unusually wet, and the three last remarkably stormy.

Having observed that the difference between the first and the other guages varied with the more or less wind, its velocity has been registered from observation; but not having an accurate anemometer, we cannot yet offer any certain conclusion further than this, that the difference in the quantity of rain received in a guage placed on the top of a building and one at a level with the surface of the ground, is, for some reason or other, *proportioned to the velocity of the wind;* and that the average excess of the lower guage is much greater than can be attributed to any or all the causes hitherto assigned. For admitting all that can be due to the difference of the sine of the angle of inclination at which the falling drops may reach the earth; and also all that could accrue from a continued condensation of aqueous vapour between the altitude of the upper guage and the surface of the ground, yet the aggregate of both would, in an elevation of only 45 feet, be trivial, in comparison of the enormous difference found every month, and on the average of the whole year.

The facts obtained do not yet, perhaps, warrant the positive conclusion, and we, therefore, offer it only as a conjecture that the aforesaid difference is owing *chiefly* to the *whirl* or *eddy* occasioned by the recoil of the gusts of wind striking on the sides of the building—an effect very visible in the disturbance of smoke issuing from chimneys during a high wind.

Since the discovery of the self-registering thermometer, and the consequent notation of the daily maximum and minimum of temperature, it has been found that the annual mean heat of the north and south of Great Britain is much more equal than was supposed; and it seems probable that the annual mean of the fall of rain will be found to differ much less than hitherto recorded. Nothing will contribute more to ascertain the fact than an uniformity of guages, as well in *situation*, as size and shape. It is desirable that in all cases there should be a guage on the level of the surface of the ground. A pit must be prepared just fitted to the bottle in which it may so stand, that the edge of the funnel shall be but half an inch above the surface, and care taken that the rim of the basin be truly horizontal. If any obstacle to the free course of the wind occurs within 100 or 200 yards of the guage, its height, breadth, and direction, should be noticed; and in respect of those placed on the tops of buildings, the *length of pipe* between the funnel and receiver, and whether within or without the house, should be mentioned; as well as the height of the guage above the ground, and above the level of the sea. It is also desirable that the barometrical tables should be always reduced to the temperature of 32°, or, if not, that the omission should be stated; and the thermometrical tables which give the true *maximum* and *minimum* of every 24 hours are preferable to the observations of fixed periods, which very often fail to show either.

I am, Sir, your most obedient servant,
H. BOASE.

ARTICLE VI.

On Finding the Sines of the Sum and Difference of Two Arcs.
By Mr. James Adams.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Stonehouse, near Plymouth, June* 8, 1822.

IT having occurred to me that by making a small alteration in the methods given by Mr. Leslie in his Geometry, and by Mr. Woodhouse in his Trigonometry, the demonstrations of the two fundamental formulæ for compound arcs may be rendered still more simple than those usually given, I will thank you to insert the following in the *Annals of Philosophy*, when convenient. I am, Sir, your most obedient servant,

JAMES ADAMS.

To find the Sine of the Sum of two Arcs.

Let the quadrilaterals A B D E be inscribed in a circle and semicircle, whose centres are C, and diameters A E, the diagonals of which being A E, BD, in fig. 9 (Pl. XIII), and A B, D E, in fig. 10. Bisect the arcs E B, E D, B D, in the points *t*, *r*, *w*; and draw the radii C *t*, C *r*, and C *w*, which will

(3 and 30 . 3 . *e* .) bisect at right angles the chords E B, E D, B D, in the points m, v, n; then will (31 . 3 . *e* .) the triangle A B E be similar to C m E, and the triangle A D E similar to C v E; therefore (4 . 6 . *e* .) A B is the double of C m, and A D the double of C v.

We then have in fig. 10, by Prop. D, Simson's Euclid,

$$AB \times DE + AD \times BE = AE \times BD \ldots\ldots\ldots\ldots (a)$$

Or,

$$2Cm \times 2Ev + 2Cv \times 2Em = 2 \times 2Bn \text{ (radius unity.)}$$

Or,

$$Cm + Ev + Cv \times Em = Bn = Dn.$$

Therefore,

$$\text{cos. } Et \text{ sin. } Er + \text{cos. } Er \text{ sin. } Et = \text{sin. } Bw = \text{sin. } rt = \text{sin. } (Er + Et).$$

To find the Sine of the Difference of two Arcs.

We have in fig. 2, by Prop. D, ibid.

$$AD \times BE + AE \times BD = AB \times DE.$$

Therefore,

$$AB \times DE - AD \times BE = AE \times BD.$$

From whence, see equation (a), we have

$$\text{cos. } Et \text{ sin. } Er - \text{cos. } Er \text{ sin. } Et = \text{sin. } Bw = \text{sin. } rt = \text{sin. } (Er - Et).$$

If from A and E, the extremities of the diameters A E, the perpendiculars A x, E s, be drawn to the chords B D, we shall, by a method equally as simple as the preceding, be able to find the cosines of the sum and difference of two arcs.

For by Prop. C. 6, Simson's Euclid, we have the following equations; viz. $AB \times AD = Ax \times AE$, and $Es \times AE = EB \times ED$ in both the figures, from whence we get

$$AB \times AD - EB \times ED = AE\,(Ax - Es) = 2Cn \times AE, \text{ fig. 1} \ldots (b)$$

$$AB \times AD + EB \times ED = AE\,(Ax + Es) = 2Cn \times AE, \text{ fig. 2} \ldots (c)$$

Or,

$$2Cm \times 2Cv - 2Et \times 2Ev = 2 \times 2Cn \text{ (radius unity).}$$

Or,

$$Cm \times Cv - Et \times Ev = Cn.$$

Therefore,

$$\text{cos. } Et \text{ cos. } Er - \text{sin. } Et \text{ sin. } Er = \text{cos. } Bw = \text{cos. } rt = \text{cos. } (Er + Et),$$ by equation (b).

In like manner, we have

$$\text{cos. } Et \text{ cos. } Er + \text{sin. } Et \text{ sin. } Er = \text{cos. } Bw = \text{cos. } rt = \text{cos. } (Er + Et),$$ by equation (c).

The arcs E B and E D, as well as their halves E t and E r, are *supposed* to be of the same magnitude in both the figures.

From the foregoing, we readily obtain the following equations; viz. $\text{sin. } (A + A) = \text{sin. } 2A = \text{sin. } A \text{ cos. } A + \text{sin. } A \text{ cos. } A = 2 \text{ sin. } A \text{ cos. } A$. $\text{cos. } (A + A) = \text{cos. } 2A = \text{cos.}^2 A - \text{sin.}^2 A \ldots\ldots\ldots\ldots\ldots\ldots (d)$

$\sin.(A - \frac{1}{2} A) = \sin. \frac{1}{2} A = \sin. A \cos. \frac{1}{2} A - \sin. \frac{1}{2} A \cos. A \ldots\ldots\ldots (e)$

$\cos. (A - \frac{1}{2} A) = \cos. \frac{1}{2} A = \cos. A \cos. \frac{1}{2} A + \sin. \frac{1}{2} A \ldots\ldots (f)$

From equation (e) we have $\sin. \frac{1}{2} A\ (1 + \cos. A) = \sin. A \cos. \frac{1}{2} A$; therefore, $\cot. \frac{1}{2} A = \frac{1 + \cos. A}{\sin. A}$. From equation (*f*) we have $\cos. \frac{1}{2} A\ (1 - \cos. A) = \sin. A \sin \frac{1}{2} A$; therefore, $\tan. \frac{1}{2} A = \frac{1 - \cos. A}{\sin. A}$; by adding and subtracting the two last equations, we obtain $\tan. \frac{1}{2} A + \cot. \frac{1}{2} A = 2 \operatorname{cosec.} A$; and $\cot. \frac{1}{2} A - \tan. \frac{1}{2} A = 2 \cot. A$. From whence $\operatorname{cosec.} A = \cot. \frac{1}{2} A - \cot. A = \tan. \frac{1}{2} A + \cot. A$; and $\frac{\cot. \frac{1}{2} A + \tan. \frac{1}{2} A}{\cot. \frac{1}{2} A - \tan. \frac{1}{2} A} = \sec. A$; or $\frac{\cot. A + \tan. A}{\cot. A - \tan. A} = \sec. 2 A$.

We also have by the common property of sines and cosines $\cos.^2 \frac{1}{2} A + \sin.^2 \frac{1}{2} A = 1$, and by equation (*d*) $\cos.^2 \frac{1}{2} A - \sin.^2 \frac{1}{2} A = \cos. A$; by adding and subtracting the two last equations, we have $\sin. \frac{1}{2} A = \sqrt{\frac{1 - \cos. A}{2}}$, and $\cos. \frac{1}{2} A = \sqrt{\frac{1 + \cos. A}{2}}$.

ARTICLE VII.

On the Use of Tincture and Brazil Wood in distinguishing several Acids, and on a new Yellow Colour obtained from it. By M. P. A. de Bonsdorff.*

It is well known that Brazil wood, when treated with an alkaline solution, yields a very fine violet colour. It is on account of this property that the tincture of Brazil wood, or paper coloured by it, is used in chemistry as a very delicate test of the alkalies.† Besides this property, it possesses another which may prove interesting to the chemist; it may be seen by the experiments which I had occasion to make on this substance, and which are the subject of this memoir, that Brazil wood paper may be employed not merely as a delicate test of the presence of acids in general, but as a certain means of detecting several acids, and distinguishing them from each other.

With respect to the action of acids upon the red colour of Brazil wood paper, it is to be observed; first, that a concentrated acid produces an alteration of colour, which is sometimes similar in various acids, but which is most commonly different from that produced by the acid diluted with water; secondly, different acids produce their effects in different times; and thirdly, the colour produced by the action of different acids is more or less durable, and it undergoes changes in a shorter or longer time. It is with respect to these variations that we now propose to consider the acids; and to give the reader an opportunity of judging by comparison, we shall successively enumerate the most common acids, and even those which do not possess a very marked action.

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* From the Annales de Chimie et de Physique.

† The chemists of France and England prefer reddened litmus or turmeric paper to detect an excess of alkali; but these reagents, and especially the latter, cannot be compared as to sensibility with Brazil wood paper.

Concentrated sulphuric acid, or when mixed with three parts of water, immediately gives a bright rose colour to Brazil wood paper, which, by attracting moisture from the air, becomes orange-coloured. When diluted with rather more water, the acid produces a colour which has a shade of yellow; and with 20 or 30 parts of water, it gives in one minute a yellow, or rather a yellowish colour, which very soon fades.

Nitric and muriatic acids act nearly in the same way as sulphuric acid, excepting that the yellow colour produced by these acids, when diluted with water, is paler; the rose colour produced by the concentrated nitric acid soon becomes yellow and greyish, and that produced by muriatic acid still sooner becomes of a dirty grey colour. The action of the three acids mentioned differs but little; but it may be employed to distinguish to a certain extent the degree of concentration of these acids.

Sulphurous acid gas bleaches the moistened paper perfectly.

Hydriodic acid, when concentrated, gives a rose colour, which gradually becomes yellow on the edges, and in a few days entirely yellow. When diluted with water, it gives a fine yellow colour in half a minute, which soon begins to fade; in a few hours, it becomes less evident, and is rather red than yellow.

Iodic acid immediately gives a pale dirty yellow colour, which remains unchanged.

Concentrated fluoric acid, whether pure or combined with silica, gives a bright red colour; when diluted, it acts in a very marked manner; it immediately becomes a fine lemon yellow colour, which disappears in the space of a minute, and soon leaves a greenish grey colour, which, by transmitted light, appears of an olive-green. When the fluoric acid is used in the gaseous state, it is sufficient to subject the moistened Brazil wood paper to its action for a few seconds. The paper is then stained a bright yellow, which disappears in the manner already mentioned. This also takes place with other volatile acids.

Fluoboric acid acts in the same way as the fluoric.

Boracic acid has no immediate action, but the colour of the paper soon becomes pale, and is eventually of a reddish-white colour. If the boracic acid contains any trace of sulphuric acid, which is always the case when it is not purified by repeated crystallization, it immediately occasions a very distinct yellowish colour, which soon disappears. The boracic acid of the Isle of Volcano acts very distinctly like pure boracic acid.

Concentrated phosphoric acid gives a rose colour, by absorbing moisture from the air, it is slowly changed to orange colour. The acid, when diluted with 10 to 30 parts of water, gives in half a minute a very fine yellow colour, which remains without any alteration.

Phosphatic acid cannot be distinguished by its action from phosphoric acid.

Concentrated phosphorous acid gives a rose colour, which becomes of a yellow colour sooner than either of the last mentioned acids, and it resembles the colour produced by those acids when diluted. Dilute phosphatic (phosphorous?) acid gives a fine yellow colour, which soon becomes pale.

Concentrated hypophosphorous acid gives also a red colour, which becomes gradually pale-yellow, and eventually almost colourless; when diluted with water it gives at first a yellow colour nearly as fine as that of the three last named acids, but it soon disappears, and there remains an indistinct colour, which is neither red nor yellow.

Concentrated arsenic acid produces a rose colour which remains for a long time. Diluted with 10 to 30 parts of water, it gives in one minute a very fine yellow colour, but in a few minutes, it fades, and becomes and remains pale-yellow.

Arsenious acid has no marked action.

Concentrated acetic acid gives a dull-yellow colour directly, which disappears immediately, and is succeeded by a pale-violet colour, which, by transmitted light, is of a very deep reddish-violet colour. Diluted with more or less water, it gives at first a yellowish colour, and afterwards, both by transmitted and reflected light, a reddish-violet colour. It is to be observed that the reddish-violet colour does not become very evident for half, or sometimes one hour, and after some hours have elapsed, the tint is still stronger; it then becomes almost as deep as the colour produced by the alkalies. If the acetic acid is not pure; if, for example, it contains sulphurous or sulphuric acid, which sometimes occur in acetic acid as usually prepared, their presence is easily detected by Brazil wood paper. Sulphurous acid destroys the action of acetic acid, or renders it extremely weak, according to the quantity which it contains, and sulphuric acid causes the acetic acid to give a yellowish colour, instead of the reddish violet. By this method, very small quantities of sulphuric acid may be discovered; acetic acid, for instance, which contains only 0·005 of sulphuric acid, gives a very evident yellowish colour.

Citric acid, whether concentrated or diluted, gives a fine yellow colour, which is as durable as that occasioned by phosphoric acid.

Tartaric acid also gives a very fine yellow colour, but it soon fades and becomes dull, in proportion to the weakness of the acid. If, for instance, it is diluted with five parts of water, it gives a less lively colour than citric acid mixed with 15 or 20 parts of water.

Malic acid acts nearly like tartaric acid.

Concentrated oxalic acid produces an orange colour which becomes gradually yellow. Diluted with one part of water, it gives a yellow colour, which remains pretty good. If the acid is diluted with three parts and more of water, the yellow colour at first produced disappears in a few minutes.

Succinic acid gives a yellowish colour which soon fades, and benzoic acid has scarcely any action.

It occurred to me that the fine yellow colour which succeeds the red colour of Brazil wood, when it is subjected to the action of phosphoric or citric acid, might be employed in the art of dyeing. To ascertain this, I tried at several times to dye wool by means of the above-mentioned substances, and these trials afforded results which exceeded my expectation. Some woollen manufacture dipped into a boiling bath of Brazil wood, acquired a yellowish-red colour, but it was dull. If, after washing and draining, it is dipped for a few minutes in a boiling and very dilute solution of phosphoric acid, or lemon juice, diluted with water, a very bright yellow colour is immediately produced.

As phosphoric acid is too dear a substance to be employed with advantage in dyeing, I tried as a substitute acidulous phosphate of lime obtained by treating bones with sulphuric acid, and I found that this substance acted precisely in the same manner, and gave as fine a colour as that produced by pure phosphoric acid. Woollen manufacture dyed yellow, by means either of an acidulous phosphate or lemon juice, may be subjected to the strongest soaping, without the colour undergoing any alteration.

I have had no opportunity of ascertaining by direct experiment whether this colour is permanent, and resists the action of the sun; it may be admitted that even if the colour produced by the action of lemon juice is not permanent, that given by the acidulous phosphate, on the contrary, will be so, as being a combination of the colouring principle with a substance which is perfectly unalterable by water, air, or heat.

Silk is also susceptible of receiving a fine yellow colour by the process which has been described; but as to cotton and linen, the very incomplete experiments which I have had an opportunity of performing, have not afforded a satisfactory result; it might, perhaps, be possible to succeed if the substance to be dyed was previously animalized. It is, however, worthy of remark, that paper, as I have already observed, receives and retains this colour with all its brightness.

Article VIII.

Astronomical Observations, 1822.
By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Occultations of small stars by the moon.

May 23.	Immersion...	8^h	59′	10·3″	Mean time. Siderial Time at Bushey.
24.	Emersion.......................	13	45	21·0	
26.	Immersion......................	15	30	08·1	

N. B. The observation of the 24th uncertain to five seconds.

Article IX.

An Investigation of the Method for finding the Sum of all the Coefficients in the Expansion of a Multinomial. By Mr. S. Jones.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Nash Grove, Liverpool, May* 29, 1822.

It is a remarkable coincidence that two of our most general theorems in analytical calculations should have been published by their respective authors, Dr. Brook Taylor, and Sir Isaac Newton, without their demonstrations: the utility of the former in the differential and integral calculus, vanishing fractions, the higher mechanics, &c. has induced many of the continental, as well as several of our own mathematicians, to attempt to demonstrate it from first principles; it is, however, generally acknowledged that its difficulty excludes that conciseness, perspicuity, and native simplicity, which all fundamental propositions ought to possess; the latter, or binomial theorem, though less difficult, was, at the time of its discovery, no easy task to demonstrate; accordingly we find that it caught the attention of the most eminent mathematicians, and has employed the talents of Maclaurin, Simpson, Demoivre, Euler, Lagrange, Woodhouse, and others, to whose minute researches and amplitude of remark it might be supposed that nothing more could be added; yet it is presumed that to demonstrate generally what only one of these, Euler has done for a particular case, and that by numeral induction, will not be thought an inelegant appendage to this beautiful theorem, nor unworthy the attention of the mathematicians of the present day.

For the sake of simplicity, I shall divide the proposition into the following cases, beginning with the easiest.

1. To find the sum of all the coefficients in the expansion of a monomial a, to the power of n.

It is manifest that $a^n = a \times a \times a \times \ldots\ldots$ to n terms, and since a has unity for its coefficient, the sum will be $1 \times 1 \times 1 \times 1 \ldots\ldots$ to n terms $= 1^n$; therefore, the sum of all the coefficients in the expansion of a monomial is 1^n.

2. To find the sum of all the coefficients in the expansion of a binomial $a + b$, to the power of n.

By the binomial theorem, $(a + b)^n = a^n + n \,.\, a^{n-1} b + n \,.\, \frac{n-1}{2} \,.\, a^{n-2} b^2 + n \,.\, \frac{n-1}{2} \,.\, \frac{n-2}{3} \,.\, a^{n-3} b^3 + \&c.$; and since b has unity for its coefficient, all the powers of b will have unity for their coefficients, which consequently will not affect their sum; whence they may be rejected, and the sum of all the coefficients in the expansion of $(a + b)^n$ will be $a^n + n\, a^{n-1} + n \,.\, \frac{n-1}{2} \,.\, a^{n-2} + n \,.\, \frac{n-1}{2} \,.\, \frac{n-2}{3} \,.\, a^{n-3} + \&c.$; but a being a monomial, the sum of all the coefficients in a^n is 1^n,

a^{n-1} is 1^{n-1},
a^{n-2} is 1^{n-2},
a^{n-3} is 1^{n-3}, &c.

These values of a^n, a^{n-1}, a^{n-2}, &c. being substituted in the expansion, gives $1^n + n \,.\, 1^{n-1} + n \,.\, \frac{n-1}{2} \,.\, 1^{n-2} + n \,.\, \frac{n-1}{2} \,.\, \frac{n-2}{3} \,.\, 1^{n-3} + \&c.$ but this series is absolutely the expansion of $(1 + 1)^n = 2^n$; therefore the sum of all the coefficients in the expansion of a binomial $a + b$, to the power of n, is 2^n.

3. To find the sum of all the coefficients in the expansion of a trinomial $a + b + c$ to the power of n.

To obtain this, put $a + b = x$; then $(a + b + c)^n = (x + c)^n = x^n + n \,.\, x^{n-1} c + n \,.\, \frac{n-1}{2} \,.\, x^{n-2} c^2 + n \,.\, \frac{n-1}{2} \,.\, \frac{n-2}{3} \,.\, x^{n-3} c^3 + \&c.$; and because c has unity for its coefficient, all its powers may be rejected, and the sum of all the coefficients in the expansion of a trinomial will be $x^n + n \,.\, x^{n-1} + n \,.\, \frac{n-1}{2} \,.\, x^{n-2} + n \,.\, \frac{n-1}{2} \,.\, \frac{n-2}{3} \,.\, x^{n-3} + \&c.$; but since x is a binomial, the sum of all the coefficients in $x^n = (a+b)^n$ is 2^n,

$x^{n-1} = (a + b)^{n-1}$ is 2^{n-1},
$x^{n-2} = (a + b)^{n-2}$ is 2^{n-2}, &c.

These different powers of 2 being substituted for x^n, x^{n-1}, x^{n-2} &c. gives $2^n + n \,.\, 2^{n-1} + n \,.\, \frac{n-1}{2} \,.\, 2^{n-2} + n \,.\, \frac{n-1}{2} \,.\, \frac{n-2}{3} \,.$

2^{n-3} + &c. which series is the developement of the binomial $(2+1)^n = 3^n$; therefore, the sum of all the coefficients in the expansion of a trinomial $a + b + c$, to the power of n, is 3^n.

4. To find the sum of all the coefficients in the expansion of a multinomial of m terms, to the power of n.

Let p = the last term, and y = the sum of all the remaining terms; that is, of the $m - 1$ terms; then the nth power of the multinomial will be expressed by $(y + p)^n = y^n + n \cdot y^{n-1} p + n \cdot \frac{n-1}{2} y^{n-2} p^2$ + &c.; but it follows, *à priori*, that the sum of all the coefficients in this expansion will be $y^n + n \cdot y^{n-1} + n \cdot \frac{n-1}{2} \cdot y^{n-2}$ + &c.; and from the preceding cases, it is manifest that the sum of all the coefficients in

y^n is $(m - 1)^n$

y^{n-1} is $(m - 1)^{n-1}$,

y^{n-2} is $(m - 1)^{n-2}$, &c.

which substituted for y^n, y^{n-1}, y^{n-2}, &c. gives $(m - 1)^n + n (m - 1)^{n-1} + n \cdot \frac{n-1}{2} \cdot (m - 1)^{n-2}$ + &c.; but this series arises from the developement of $(\overline{m - 1 + 1})_n = m^n$; therefore, the sum of all the coefficients in the expansion of a multinomial of m terms to the power of n is m^n.

Throughout the preceding investigation, the exponent n has been taken arbitrarily, it may, therefore, be expounded by any number whatever, either positive or negative, whole or fracted.

I am, Sir, yours truly,

S. Jones.

Article X.

Observations on the Presence of Moisture in Modifying the Specific Gravity of Gases. By C. Sylvester, Esq.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *60, Great Russell-street, June 5, 1822.*

Whatever Mr. Herapath may say of Dr. Thomson's paper, its foundation is good, and the principal facts from which his conclusions are drawn have been long known to the philosophical world, and confirmed by experience; I allude particularly to the fact of the same weight of steam at all temperatures containing the same quantity of heat; and that the sum of the degrees expressive of the latent and sensible heat is a constant quantity. He is doubtless wrong in making these sums commence at 32°. If the principle be true above that degree, it must be equally applicable to those below the same. That is, if l be the latent heat, and t the temperature, $l + t = c$ a constant quantity, whatever t may be. I differ with Dr. Thomson as to any practical advantage derived from the variable quantity of latent heat at different temperatures either in distillation or in its agency in steam engines. Suppose in the former application that vapour is distilled over at the temperature of 70°, and condensed in a temperature of 50°, a constant succession of liquid will be formed by condensation, which is the practical effect desired, and it must be admitted from the law quoted by Dr. Thomson, that the stock of vapour at 70° constantly passing from the still to the receiver will hold more latent heat than the same quantity at a higher temperature. The difference will consist in having a small excess of latent heat, which is "in the uncondensed vapour at every period of the process without any disadvantage," as to the ultimate quantity of liquid condensed. I am inclined to think that if the size of the apparatus be increased so that the same weight of vapour may come over in the same time, the advantage would be in favour of the low temperature, owing to the quantity of heat lost in all processes carried on at high temperatures by radiation and the conducting power of contiguous bodies.

For the same reasons there is no advantage in using steam for engines at a high pressure. Whatever may be the fuel consumed to make a given volume of steam equal to one atmosphere, it will take twice the quantity to give twice that volume, or the same volume of a density to give a pressure equal to two atmospheres. I should think therefore, that the increased temperature of the volume equal to two atmospheres would lose more heat to surrounding bodies than the two volumes of one atmosphere, but the mechanical advantage of the two will be obviously the same. The boasted advantage of the Cornish engines has chiefly arisen from their inventor assuming some erroneous data respecting the power of steam, and many others, even Mr. Herapath, seem to have fallen into the same mistake. In the range of temperature commonly used for high pressure steam, it will be found that from an increase of temperature of every 30° degrees, the density and elasticity of the steam become doubled; that is, at 212°, its elasticity is equal to about 30 inches of mercury, and a cubic foot of such steam would weigh about 253 grains. At $212 + 30 = 242$ degrees, the volume remaining the same, it will support 60 inches of mercury, and a cubic foot will contain $253 \times 2 = 506$ grains. Hence it will appear that the temperature is increasing in arithmetical proportion while the power of the steam increases in geometrical proportion, and hence the apparent advantage by working with high pressure. *

The source of this fallacy will be found in the assumption of

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* The force of steam has not strictly a geometrical ratio to the temperature. The ratio for 10° below 212° is about 1·23. And this ratio for every ten degrees above *will decrease by ·01,* while steam, for every ten degrees below, has a similar increase.

the quantity of heat being as the temperature; when the fact is, that while the temperature has been advancing by 30°, the real quantity of heat is doubled; and it will be found that a cubic foot of steam of 60 inches pressure in mercury, although only 30° in temperature above a cubic foot of 30 inches pressure will heat twice the quantity of water to the same temperature, or melt twice the quantity of ice, which is the clearest proof that their respective quantities of heat are as 2 to 1.

The remaining part of this paper which is applied to calculate the correction for the specific gravity of gases, as affected by the presence of aqueous vapour, is very valuable. If the force of aqueous vapour at different temperatures be correctly taken in order to get the specific gravity of the same, nothing can be more simple than the formula given by Dr. Thomson for finding the allowance to be made for the presence of vapour in any gas. This is the same formula which is contained in my paper sent to your journal for finding the proportions of mixed inflammable gases. I remain, dear Sir, yours very truly,

C. SYLVESTER.

ARTICLE XI.

Extracts from the "Journal of a Survey to explore the Sources of the Rivers Ganges and Jumna."* By Capt. J. A. Hodgson, 10th Reg. Native Infantry.

As I have had it in my power to explore and survey the course of the Ganges within the Himālaya mountains to a considerable distance beyond Gangautri, and to the place where its head is concealed by masses of snow which never melt, I hope, that an account of my journey may be acceptable to the Asiatic Society. I must premise that, as Capt. Raper's account of Capt. Webb's survey in 1808, has already appeared in the eleventh volume of the Researches, I have nothing to add to that officer's able and faithful description of the mountainous country, passed through in the route of the survey from the Dūn Valley to Cajani, near Reital, where the survey towards Gangautri was discontinued in consequence of the serious obstacles which impeded it. I shall, therefore, only give an account of the course of the river above the village of Reital, where I halted to make arrangements for my progress through the rugged regions before me, in which I found I had no chance of getting any supplies of grain for my followers: I was consequently obliged to buy grain, and to send it off before me, so as to form little magazines at the places I intended to halt at; and as I learned that several of the sangas or spar bridges over the river had been destroyed by avalanches of snow, I sent a large party of labourers to re-establish them.

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* The Editor is favoured with these extracts from almost the only copy of Captain Hodgson's Journal, which has reached England, by Mr. Edmonstone, of Newcastle; who observes, that in order to shorten the communication, a number of minute and interesting details have been necessarily omitted. This circumstance will serve to explain the breaks which the narrative occasionally assumes, and we should hope will be received as a sufficient apology for our not doing all the justice that we could wish to the labours of Capt. Hodgson, who has since been appointed to the important situation of Surveyor-General of India.

Considering Reital as a point of departure, it will be satisfactory to know its geographical position. By a series of observations with the reflecting circle of Troughton, and also by his astronomical circular instrument, I found the latitude to be 30° 48′ 28″ N.; and having been so fortunate as to get two observations of immersions of the first satellite of Jupiter, and one of the second, I am able to give a good idea of the longitude of the place; and the more satisfactorily, as two of the immersions are compared with those taken at the Madras Observatory on the same night, and with which I have been favoured by Mr. Goldingham, the astronomer there.

The telescope used by me in observing the satellites was a Dollond's 42 inches achromatic refractor, with an aperture of two and three-quarter inches, and power of about 75 applied, having a tall stand, and rack work for slow motion. The watch was a marine chronometer, made by Molineux, of London, and went with the greatest steadiness on its rate, as nightly determined by the passage over the meridian of fixed stars observed with a transit instrument. The time of mean noon when required was always found by equal altitudes.

By a mean of several observations taken at Madras about the time of four immersions of the first satellite, which I observed at Mr. Grindall's house near Seharanpūr. (Mr. Goldingham finds 5h 10′ 24″ for the longitude of Seharanpūr.) A snowy peak called Srī Cānta is visible both from Reital and Seharanpūr, its position is determined by means of a series of triangles instituted by me for the purpose of taking the distances and heights of the snowy peaks. I find the angle at the pole or difference of longitude between Seharanpūr station and Srī Cānta to be 1° 14′ 47″, the peak being east, and at Reital, the difference of longitude of that village, and the peak is found to be 12′ 6″; the peak being east, consequently the difference of longitude of Seharanpūr and Reital is 1° 2′ 41″. On the whole, I think 5h 14′ 20·6″, or 78° 35′ 60·7″ may be safely taken for the longitude of Reital, east of Greenwich.

Reital contains about 35 houses, and is esteemed a considerable village; as is usual in the upper mountains where timber is plentiful, the houses are large, and two or three stories high. When a house has three stories, the lowest serves to shelter the cattle by night; the second is a sort of granary, and in the *upper* the family dwells; round it there is generally a strong wooden gallery, or balcony, which is supported by beams that project from the walls. The roofs of the houses are made of boards or slates; they are shelving, and project much beyond the top of the walls, and cover the balcony, which is closed in bad weather by strong wooden shutters or pannels. These houses are very substantial, and have a handsome appearance at a distance, but they are exceedingly filthy within, and full of vermin. The walls are composed of long cedar beams, and stone in alternate courses, the ends of the beams meet at the corners, where they are bolted together by wooden pins. Houses of this construction are said to last for ages; for the Deodar or Cailon pine, which, I suppose, to be the cedar of Lebanon,* is the largest, most noble and durable, of all trees.

The situation of this village on the east side of a mountain, the summit of which is covered with snow, and the foot washed by the Bhágirathé is very pleasant. It commands a noble view of the Srī Cānta, and other adjoining peaks of the Himālaya, on which the snow for ever rests. Snow also remains until the rains, on all the mountains of the second order, which are visible hence, both up and down the river. Many cascades are formed by the melting of the snows on the foot of the surrounding mountains. One in particular descends in repeated falls of several hundred feet each, from the summit of a mountain across the river, and joins it near Batheri.

In the following account of my progress up the river, I have put down such remarks as occurred at the time, and they were written on the spot, and are here inserted with very little alteration. Though I am aware that such minute descriptions of localities must appear tedious, I hope they will be excused by those who, feeling interested in the subject, may have the patience to read the detail. To give *general* descriptions of such rude regions is difficult, if not *impossible*, and I trust that particular ones, though often tedious, will be found more faithful, and to give more precise ideas of those remote recesses of the Himālaya, which I visited.

On the 19th of May, I was joined at Reital by Lieut. Herbert, of the 8th Reg. N. I. who had been appointed my assistant, and from his skill and zeal, the survey has received much benefit.

Mr. Herbert came direct from Calcutta, and brought me a pair of mountain barometers, but the tubes filled in England had been broken before they arrived in Calcutta: there were some spare empty tubes which we filled and used as hereafter mentioned, but we could not succeed in boiling the mercury in the tubes to free it entirely of air. The height of Reital above the sea, as indicated by our barometers, is 7108 feet.

Having received reports that the sanghas were repaired, and that the grain I sent forward was lodged in the places I directed,

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* It is *the pinus Deodára of Roxburgh*; the Dévadáru of Sanscrit writers.—H.H.W.

I left every article of baggage I could possibly do without, and having given very light loads to the coolies that they might proceed with less difficulty, we marched from Reital on the 21st of May.

On the 27th we reached the Soar river, from whence to immediately above Tawarra, the path is exceedingly rugged, over broken masses of rock. The whole is an ascent; and in some places, very steep open precipices to the right, and high rocks above to the left; precaution is required in the footing, and some places are very unpleasant to turn, where it is adviseable to go bare footed.

The mountains are of granite, with various proportion of quartz and feldspar, of which I have specimens. Heavy rain both on going and returning; could not get a latitude. Water boiled at 198°, the temperature of the air being 67°.

At the village of Tawarra, direction of the small lake called Cailac Tāl, whence the Dinni Gārh river issues 71°. It is said to be 50 yards in diameter, but deep, and is formed by the melting snow; there is a small piece of level ground near it to which the villagers drive their sheep to pasture in August.

Descent through the fields and down the Dell steep and slippery. Rhoh (or Rhai) pines and the mohora, a species of oak, grow here.

Descent to the Elgie Gārh torrent; cross it by a sangha 15 feet long. Granite rock in large blocks, with quartz nodules and bands in the bed of the stream.

Cross Camaria Gādh (rivulet), eight paces wide.

Down the narrow glen of the rivulet to its junction with the Ganges; the whole a descent, and in many places bad and difficult, over large blocks of rock which have fallen from above, and overturned and shattered all the trees in their course. The granite precipices which confine the river at this place have split and fallen in large masses into the bed of the stream.

Path along the side of the Ganges, but above it a cascade opposite, falls 800, but not in one sheet, river up to 6°; path rocky.

Across the river and on its steep bank is a range of hot springs; they throw up clouds of steam, and deposit a sediment of a ferruginous colour; these are the first hot springs I have observed on the Ganges; the river not being fordable, we cannot go to them.

Huge blocks of rock fallen to left.

Climb over and under the ruins of a most tremendous fall of the precipices; blocks of granite from 100 to 150 feet in diameter are thrown on each other in the wildest and most terrific confusion; the peak whence they fell is perpendicular, and of solid rock. This fall took place three years ago.

Cross the Ganges by a sangha made of two stout fine spar laid from rock to rock. It is a good bridge of the kind, and about 3½ feet wide; the space between the pine spars is overlaid with small deal shingles which are tied together so as to form a platform. Like all the rest, this sangha is open on both sides, and unpleasant to pass, being from the length and elasticity of the pines so springy as to rebound to every step the passenger takes. The river below the sangha was deep, and very rapid, being confined by rocks. Its breadth under the sangha, as measured by a chain, was 50 feet; height of the sangha above the stream 30 feet. The river is more expanded above and below. Sanghas are always placed in the narrowest parts.

Tent at Dangal, a small flat so called on the left bank of the Ganges, and at the confluence of the Limea, a large torrent. No village here. The halting place is surrounded by high and steep rocky mountains, and mural precipices; observed some bears climbing among the rocks.

Time of marching five hours and 48 minutes; a very laborious journey. The path is very rough, and merely a succession of steps from one broken crag to another; some places very difficult. To the Ganges was descent; then we passed along its bank at no great height above the stream, which, though not wide, is deep and impetuous, falling from rock to rock. In the less rapid parts pools are formed, where the breadth may be 200 feet, but generally it appears from 100 to 120 feet wide; several rills besides those noted above, fall into the river; it is needless to say, that they fall in cataracts, the sides of the river being every where bounded by high cliffs. The rocks are granite, of much the same composition as on yesterday's march. The dip of the strata is about 45° towards NE. as usual, and the whole line of inclination is visible from the river to a great height above. Water boils at 202°, the temperature of the air being 54°. On our return, the barometer was deranged at this place. It is to be remarked that on going up, we did not fill the barometers, fearing they might be broken, and the mercury spilt, of which we had very little; our store of it having been diminished by those various accidents to which every thing that can be lost or broken in these rough regions is subject. Of these barometers more hereafter.

Lofty cliffs on both sides of the river; path generally a slight ascent, but rocky and difficult.

Narai peak crowned with snow.

Cross the river on a sangha at Deorāni Ghāti; it is a new and good bridge of the kind, but long and very elastic. Height above the stream 40 feet; breadth of stream under the sangha 30 paces, or about 60 feet. The high flood mark of the stream when swollen, appears to be about 14 feet above the present level. A wild and savage looking place. Precipice around, granite and some black and grey rock of a laminar texture. Rocky path from last station. Pines of various kinds, and the true deal fir, grow here. Immediately on passing the sangha, the path leads over an avalanche of snow which reaches to the river's margin; it is many feet thick, and has fallen this year, and brought down all the trees in its path. This is the first snow bed we passed over on the Ganges.

The river, a bed of foam falling from rock to rock. Five hundred yards further on, are the falls of Lohari Naig, where the river is more obstructed than in any part of its course, and tears its way over enormous masses of rock, which have fallen into it from the mural precipice which bounds its left shore. This frightful granite cliff of solid rock, of above 800 feet high, appears to have been undermined at its foot by the stream, and the lower and middle part have fallen into it, while the summit overhangs the base and the river. The vast ruins of this fall extend for about a quarter of a mile; the river has now forced its way through, and partly over the rocks, with a noise and impetuosity, we thought could not be surpassed; but on our return in June, when the Ganges was doubled in depth, the scene was still grander. It then just covered the tops of the rocks, and one of the falls of the whole stream we estimated at 25 feet perpendicular, and below it were more, close to each other, of little less height. The scene is full of sublimity and wildness, and the roar of the water is astounding.

On the right bank also there has been a recent large slip of the mountain, but the above-mentioned on the left bank is for its height the most formidable fall I ever saw. It is not recent.

Cross the Ganges by the sangha of Lohari Naig, 16 paces long, and 25 feet above the stream, which is here narrow, deep, and has a great fall; the ends of the sangha (which is very narrow) are supported on each side on two great tabular granite rocks: that on the right bank is circular, and 150 feet in circumference. It is of a coarse brown granite, with quartz intermixed, and is decomposing in some places. The mountains on both sides of the river are very steep. On the left bank of the river observed a rill, impregnated with calcareous matter, which is so abundant as to incrust every thing it touches very strongly, and we collected large pieces of this lime, which is pure, like that at Sansūr Dhāra. This is a singular thing in a region of granite.

The Lot Gārh river joins the Ganges; cross it by a good little sangha. This river is 20 feet wide. This last station has been almost level, and a good and pleasant path along a flat of 150 yards wide by the river side, shaded by cākṣi, mirei, omil, and other trees. From the edge of the flat, the rock rises in a gigantic mural precipice of about 1500 feet perpendicular, and the same across the river. Strata much inclined. The Lot Gārh river comes from the snow to the right, and is very rapid. Ganges here expanded, and the scenery beautiful.

On our return breakfasted here.

Barometer	23·144 in.
Thermometer attached	53°
Detached	56°

Very steep and difficult descent, open to the left, and the river deep below; a mural precipice across the river with well defined strata at an angle of 45°. The strata are so arranged in these regions, which are the feet of the Himālaya; but I have observed that near the tops of the *highest* peaks, the layers of rock are *nearly horizontal.* Name of above mountain Baldera Lūru; steep as it is, and nearly devoid of soil, the pines nevertheless contrive to fix their roots in many parts of it.

Bad and narrow path overhanging the river. The Soan Gādh (river) joins the Ganges below to west; appears to be 30 feet wide, and not fordable: very rapid.

Bad and rough; here cross the Ganges on a sangha about 45 feet above the stream; breadth of the roaring stream below 17 paces, or 42 feet. The bridge about 2¼ feet wide, ill secured, and unsteady; it extends from one large rock to another. The current extremely violent, and the fall of the river great.

A torrent from the Suci mountain falls in here; at this sangha on return, barometer, 22·90 in.; thermometer, 52°.

Long ascent to Suci, a decaying village of nine houses, of which three only are inhabited. It is on the west side of a mountain, and surrounded on all sides by the Himālaya rocky precipices crowned with snow. The river is about 1000 feet below, foaming in a confined channel.

As to the march, it was very long and laborious; we performed it in seven hours; probably one-fifth of it was hand and foot road. The rest, except the two places of flat mentioned above, as usual, a succession of long strides, or little careful steps from one broken crag to another. The three sanghas over the river, having been lately repaired, are not dangerous, but too high, narrow, and elastic, to be pleasant to cross. The people from the plains passed them very well (three persons excepted), but many of the mountain coolies were obliged to be led over with their eyes shut, as well as some of the Goorkha sepoys. To get well over them, it is proper to take careful steps (but not to go too slow), and to keep one's eyes steadily fixed on the platform, and by no means to look over the side at the foaming gulf below, or to stop or hesitate when on the sangha. The scenery to day was in Nature's grandest and rudest stile. Wall-like precipices of compact granite, bounding the river on both sides, to the immediate height of 2000 or 3000 feet; above those cliffs is snow. Latitude observed, 30° 59′ 40·25″.

Descent and cross the Ganges by a sangha; length of the bridge 115 feet, breadth 3 feet; breadth of the river below, 82 feet; depth to the surface of the water from the sangha, 19 feet (measured by the chain). This is the best sangha on the river, and the water below is not so rapid as usual, Jhala, village of five houses; above Jhala, the country is not at present inhabited.

A fine view up the river, which, for several miles above this, flows in a more expanded bed in a narrow valley; the feet of the mountains bounding it are less steep, and are clothed with cedars. Good path along sand and pebbles in the river's bed, the current of which more gentle though very swift. The bed is about 600 yards wide, and will be overflowed when the river is at its height. Lower line of snow generally 2000 feet above the river, though several avalanches reach down to its margin. The air is very cold.

We have now turned the snowy range, seen from the plains, and brought it to our right; the march from Dangal to Suci, and on to this place, may be considered as in that gorge of the Himālaya through which the river forces its way to the foot of those mountains of the second order, which are the beginning of the spurs of the grand range. We have now the great snowy peaks on both sides of the river, and it is henceforward bounded by them. Those to the right are visible from Hindustan; those across the river, or to our left, are not visible from the plains, being hid by the southern ridges. The line of the outlet of the river is very perceptible from the plains, and the Sricānta peak, the western foot of which it washes here, is conspicuous from Seharanpur and the Doab. From hence onward, the course of the Ganges is to be considered as being within the Himālaya differing from the Jumna, inasmuch as that the source of the latter river is at the south west feet of the snowy peaks seen from Seharanpur, and not within the Himālaya.

Pleasant and level; a snowy peak towards Barrasah shows itself up the Soan Gādh: it is called Dumdara, and is very white with snow: mouth of the Soan Gādh 322°. Down its bed the plunderers from Barrasah and the western districts of Rawaien penetrate in the latter end of the rains. As far as Barrasah, the country is uninhabited for six days' journey, except at Louh-panch Gong, which is three coss on this side of Barrasah. Those districts are on the Tonse river, and are the seat of numerous gangs of plunderers and murderers who much infest this part of the country.

Descent to the bed of the Ganges, and cross the Til Ghar, a large torrent which falls in a most beautiful and picturesque cascade of 80 or 100 feet over a rock, bordered and shaded by high feathery pines and spreading cedars. Flat, over sand and pebbles of the river, bed here expanded.

On our return we halted at this place to take the altitude of two very sharp snowy peaks, which now appeared to the south, or to our right. We measured carefully with the chain a base of 165 feet, which was the greatest extent of level ground to be found; with this base we found a longer line of 1568 feet, and from its extremities determined the distances of the two peaks, and their heights above the east end of the base, as follows:

First peak called Sewmarcha Chauntal, distance 16440 feet, bearing due south. Its angle of elevation 26° 43′ 42″, and height above the river 8278 feet.

Second peak no name, but it is a lower part of the Srīcānta mountain. Distance 15374 feet. Magnetic bearing 170° 43′. Angle of elevation 25° 55′ 30″. Height 7473 feet above the river. Barometer 22·249 inches; thermometers attached 79°; detached 78°.

N. B. On our return, we found gooseberries at this place; they were of the large hairy kind, and, though not ripe, made good dumplings.

Gradual descent, and cross the Kheir Gādh large rivulet by a sangha at Derāli, a village of six houses, but now deserted on account of the failure of the crops and incursions of banditti.

The road to day, considered as a mountain path, was excellent, two or three places excepted. The north bases of the mountains which we passed along are moderately steep, and are clothed with noble cedars, and various sorts of large pines, of which the cshīr and khai, or kher, are the largest. Cshīr is a name indiscriminately given to several of the large leaved pines, but the tree so called here is the true deal; it grows to a great height, and bears a resemblance to the common cshir or turpentine fir, which abounds in the lower hills, but which is never seen in company with the cedar (deodār). I took some specimens of this deal; it is light, and has a fine grain: the rhai is a lofty pine; it has a graceful appearance; the leaves are pendent. The wood of it is not esteemed for building, being heavy and knotty: the cedar is always preferred for that purpose. From the sangha to Derāli the Ganges flows in an expanded bed with a swift current over stones. Yesterday it was a succession of falls from rock to rock, and bounded by frightful precipices. To day the scenery was very interesting, the river being bounded immediately to the north by the cedar forests; above which towered the sharp snowy peaks, and many torrents and cascades fell from them. I never made a more delightful march; the climate is pleasant, and the weather bright to day. The village of Derāli is situated in a rocky recess, and commands a fine view of the river, and of the north sides of the snowy peaks behind Jamnautri. There are three small temples of stone by the river side; they are of good workmanship. Derāli was plundered last year by banditti from the westward.

Pole star hid by the mountains as usual. Crest of nearly perpendicular and difficult short ascent: crags overhanging and threatening to fall. The river bed the whole way broad, and strong current at Derāli; lofty peaks on every side rising immediately from the river. This place is 1000 feet above it. Cedars of great size here.

Road generally level on bank of the river: cross an avalanche of great magnitude, being a fall of lumps of snow like large rocks, it has brought down, and broke to pieces, all the cedar trees in its path: perpendicular rocky precipices rise immediately from the river bed to the height of 1500 and 2000 feet; high snow peaks on all sides; large cedars at their feet.

An exceedingly steep ascent; river not visible, but close below mountains with bare peaks; not a blade of herbage on their rocky sides. In front, Decani snowy peak; to our left a mountain called T'hui̅. The south side of Decani is washed by the Bhāgīrat'hī, and the north side by the Jahni Ganga or Jāhnevī, their confluence being at Bhairog'hāti. This place is called Ratenta.

Another steep and toilsome ascent.

Descent over broken fragments of peak. A rocky precipice nearly mural, of 1000 feet, overhangs the right bank of the Ganges, which here, as usual, rushes over rocks with an impetuous and foaming current. In front is the gigantic peak Decani rising immediately from the bed of the river, on the left, almost equally high, one of T'huī; below immense masses of granite overhang the river. The scenery is very grand. Very large cedars here.

A sweep from S. to E. brings us to that most terrific and really awful looking place called Bhairog'hāti.

The descent to the sangha is of the steepest kind, and partly by a ladder. The sangha is inclined far from the level, and, as seen from the height above it, cannot fail to inspire the beholder with anxiety as to his safe passage over it. It is indeed by far the most formidable sangha I have seen; the height of the platform above the river, we measured by dropping the chain; it was 60 feet. One is apt at first sight to estimate it at much more; however, this height added to the circumstances of the narrowness of the sangha (about 2¼ feet wide), its elasticity, and its inclined position, is sufficient to render its passage disagreeble, it being (like all the rest) quite open at the sides. It is laid from one side of the precipice to the other; the end on the left bank is the highest; the precipices in some places are quite perpendicular, in most, nearly so, rising to the height of 3000 feet above the stream; they are of compact granite. On some ledges there is a little soil where the cedars fix their roots. The river below the sanghais closely confined by the wall-like rocks, which are perfectly perpendicular, and its course is thus bounded nearly to Gangautri. The breadth of the stream is about 45 feet, and it is deep under the bridge.

Turn to the left by a rocky path to our tent, which is in a very strange place for a tent to be in; and one of the most curious sights among many here, is to see a little tent pitched under vast overhanging masses of rock at the confluence of these two rivers, the Bhāgīrat'hī, and its foaming rival the Jāhni Gangā, or as more properly called, the Jāhnevī. The strange and terrific appearance of this place (Bhairog'hātī) exceeds the idea I had formed of it. No where in my travels in these rude mountains have I seen any thing to be compared with this, in horror and extravagance. Precipices composed of the most solid granite confine both rivers in narrow channels, and these seem to have been scooped out by the force of the waters. Near the Sanga, the Bhāgīrat'hī has in some places scolloped out the rock which overhangs it. The base of these peaks is of the most compact sort of granite; it is of a light hue, with small pieces of black sparry substance intermixed. From the smoothness of the rocks which confine the stream, and which appear to be worn so by water, I think the stream must have formerly flowed on a higher level, and that it is gradually scooping its channel deeper; for it does not appear that the walls which confine the rivers are masses fallen from above, but that they are the bases of the peaks themselves. Enormous blocks have indeed fallen, and hang over our heads in threatening confusion; some appear 200 feet in diameter, and here are we sitting among these ruins by the fire side at noon. Thermometer 52°. What are these pinnacles of rock, 2000 or 3000 feet high, which are above us, like? I know not. To compare small with great, I think the aptest idea I can form of any thing that might be like them, would be the appearance that the ruins of a Gothic cathedral might have, to a spectator within them, supposing that thunderbolts or earthquakes had rifted its lofty and massy towers, spires, and buttresses; the parts left standing might then in miniature give an idea of the rocks of Bhairog'hātī.

The great cedar pines, those gigantic sons of the snow, fringe these bare rocks, and fix their roots where there appears to be very little soil; a few also of the larger deal pine are seen, but inferior trees do not aspire to grow here. The day is dull and rainy, and I cast my eyes up at the precipice overhead, not without awe; a single fragment might dash us to pieces.

Avalanches of snow and rock such as we have passed to day, and indeed for these three last days, show by their effects their vast powers of destruction; for they bring down forests in their overwhelming course, and dash the cedars into splinters. These avalanches have all fallen this season; they have in some places filled up the dells and water courses to a great depth with snow, and extend from the peaks to the margin of the river.

A painter wishing to represent a scene of the harshest features of nature, should take his station under the sanga of Bhairog'hātī, or at the confluence of the Bhāgīrat'hī and Jāhnevī rivers: here it is proper to take some notice of this latter river hitherto little known. Though the Bhāgīrat'hī is esteemed the *holy and celebrated Ganges*, yet the Jāhnevī is accounted to be, and I think is, the larger stream. From a Brāhman who officiates at Gangotri, and who has *been up it*, I collected some particulars, which, though, perhaps, far from correct, may serve to give an idea of it. By the course of the river is a pass to Bhoat or Thibet, by which the people from Reital and the upper villages of Rowaien go to get salt, blanket cloth, and wool, in exchange for grain. The trade is trifling, and not more than 100 people go yearly; in the latter end of the rains the road is open. They carry their goods on sheep and goats. The Brāhman has been at the frontier village called Neitang; it is four long and very difficult days' journey. The first three days are up the course of the river, high above its bed for the most part, but occasionally descending to it. It is exceedingly steep and difficult.

First day.—They go along the high precipice on the right bank of the river: a sangha at the end of a long march. Very bad path. No village.

Second day.—Having crossed, very bad path to Cartcha, a halting place. No village. Cedar pines here.

Third day.—On same bank of the river to Handouly, a halting place; but no village. Not very long march.

Fourth day.—The frontier or (do-phāshiās) village called Neitang, in the district of Tangsah; at this village, the river seems (they say) but little diminished in size, and there is a sangha over it. The Brāhman can give no account of its origin, except that he believes it comes from some hills in Bhoat. The first part of the course of the river upwards, so far as can be seen from Bhairog'hātī, is 72° NE.; and from what I can understand, it appears that this river has its source to the north of that ridge of the Himālaya which bounds the Bhāgīrat'hi to the NE. or on its right bank, and that between Bhairog'hātī, and, perhaps, the third day's march above-mentioned, it forces itself through the range. The Brāhman says that at the village, and for the last day's march to it, the mountains are bare of trees, and that they are not the Cylās mountains (i. e. not what we call snowy mountains) but that the Cylās peaks towards Gangotrī are seen to the right, and so they would be, if we suppose the course of the Jāhnevī up, to be about N. 70 E.; and the course of the Ganges is, we know from hence, considerably to the S. of E. By the way I may mention here, that cylās is a general appellation for high ranges always covered with snow, in the same way as we say Himālaya or Himāchal (which last indeed literally means snowy peaks).

At Neitang, the houses are built very low on account of the high winds. Travellers suffer much from difficulty in breathing, caused, as they say, by the bic'h or bis'h; i. e. exhalations from poisonous herbs which grow on the high bare knolls. This frontier district of Tungsah appears to be considered to belong to what they call here Bhoat or Thibet, and they pay their land tribute to a collector who comes from Chaprang. Of the distance, or size, or direction, of Chaprang, I could not get any satisfactory account, but it appears to be a Chinese dependency. The district also gives to the Rājā at Bassāhir a blanket per man every third year, and a small complimentary tribute of dāc'h (raisins) to the G'harwāl Rājā. The inhabitants are called dophāshiās from their speaking the languages of both G'harwāl and Bhoat, and they act as interpreters and brokers.

The exports from Rawaien are rice, mandwā and pāprā (coarse grains), tobacco, and tamashas. Imports, salt, and thick woollen cloth and wool,

The Rawaien people go in the month of Cārtic, because the wool is then ready, but in the month of Sāwan, the road may be passed, and that would be the best time to go.

Had the season been more advanced, and if I had had grain, I should have been tempted to go up this river; it is an interesting object of future research, but there are many others, and one does not know which to attend to first; but it is my intention to explore this river next season.

Latitude observed. Confluence of the rivers at Bhairog'hātī.

Water boiled at 198°, the air being 44°.

On our return, June 3, we encamped in a much better place, a small piece of flat at the summit of the cliff which bounds the Ganges on its left side. It was a pleasant and secure situation, and under the shade of the cedars. At this place, about 700 feet above the river, the barometer (unboiled mercury) stood at 21 inches, temperature of air 70°.

Latitude of this camp 30° 01′ 22·5″ good observations; junction of Bhāgirat'hī and Jāhnevī rivers 72° distant 1 furlong.

A very steep and difficult ascent, we pass along the perpendicular face of the precipice by means of a scaffolding of two narrow planks, which appear very rotten, and ill supported at the ends; under the scaffold is a chasm of 300 feet deep. Immediately afterwards, ascend by ladders, the precipices bounding the river, being here like walls, and these scaffolds and ladders are laid from projecting points to enable one to pass.

Three other passages along the precipices and over chasms by means of rotten planks; then an exceedingly steep ascent by short zigzags to a flat at the foot of Decanī peak; here is a small temple of Bhairo Lāl who is esteemed the janitor of Gangotrē; at this place, pious Hindūs leave their shoes.

Road tolerably level; winds round the SW. side of Decanī peak; the river is about 800 feet below to the right, and rising from its bed is a wall of mountains of a height I find it difficult to estimate; below to the river steep precipices.

Path very difficult; a few paces further on cross another frightful chasm by a platform of a foot or 18 inches wide. Road over masses of granite piled in confusion; they are fragments of a fallen peak. Looking up, we see the tower-like summits of Decanī almost overhanging us. The whole way strewed with falls of rock from them. Many traces of bears.

Wind round *the brow* of the hill, and come upon an opening where the eye is saluted with a full view of Miānrī peak, and in the distance, the mountains of Rudr Himālaya crowned by the peak of Dugdī towering to a great height; the pure snows on it shine in the sun's rays with dazzling brilliancy.

Rather better path; the river deep below, foaming in its narrow and rocky bed. Most fantastic great snow peak over Gangotrī.

Black rocky peak across the river. Call it Iron Sides.

Path as before. Across the river is a cascade falling through a large snow bed; the snow reaches in several places from the river bed on the opposite side, to the summit of the mountains which are very steep. We are almost in sight of Gangotrī.

The river flows under beds of snow which have fallen into it from the peaks, and cover it.

Pass above a cascade falling over a precipice of grey granite with black sparry spots. Wonderfully steep precipices on both sides of the river; on this side, the rocks are quite bare and shattery.

Cross above a cascade falling from a rocky gorge to the left. Path extremely bad. This river below foaming between walls of rock perfectly perpendicular. A sangha (now destroyed) had formerly been laid over at this place by the banditti, who, in the rains, plunder the Cēdārnāth districts to the eastward. The rocks through which the river flows have horizontal strata, and the light hue of Portland stone. They are as usual granite. The cedars here are poor and starved. Rudr Himālaya a snowy peak 95°. Gangotri: the small temple of Gāngā Māī and Bhāgirat'hi on right bank of Ganges.

The path to day was of the worst description, and is on the whole, I think, the most rugged march we have hitherto had, though there are not any long ascents. Nothing can be more unpleasant than the passage along the rotten ladders and inclined scaffolds, by which the faces and corners of the precipices near Bhairog'hātī are made. The rest of the way lies along the side of a very steep mountain, and is strewed with rocks. The views of the snowy peaks which are on all sides, were very grand and wild.

The rocks are of granite, but of a lighter colour than usual, and specks of a bright black sparry substance are interspersed in them at the distances of from one to three inches.

The river's bed from Bhairog'hātī to Gaurīcund, was between mural precipices of 200 or 300 feet high; above them was the steeply inclined ground, along which our path lay. Though very rocky, there were many places with soil where the cedars grew, but not large. Above the path to our left were bare rocky precipices, on the summit of which the snow lies: at Gaurīcund and Gangotrī the river's bed becomes more open. The temple at Gangotrī is a Mundup of stone of the smallest kind; it contains small statues of Bhāgīrat'hī, Gangā, &c. and it is built over a piece of rock called Bhāgirat'hī-Sitā, and is about 20 feet higher than the bed of the Ganges; and immediately above its right bank, there is also a rough wooden building at a short distance for the shelter of travellers. By the river's side, there is in some places soil where small cedars grow; but in general the margin is strewed with masses of rock, which fall from the precipices above. The falls do not appear recent.

Too much tired to attempt to boil mercury in the tubes to day. At night, having prepared the instruments to take the immersion of one of Jupiter's satellites, we laid down to rest, but between 10 and 11 o'clock were awakened by the rocking of the ground, and, on running out, soon saw the effects of an earthquake, and the dreadful situation in which we were, pitched in the midst of masses of rock, some of them more than 100 feet in diameter, and which had fallen from the cliffs above us, and probably brought down by some former earthquake.

The scene around us, shown in all its dangers by the bright moonlight, was indeed very awful. On the second shock, rocks were hurled in every direction from the peaks around to the bed of the river, with a hideous noise not to be described, and never to be forgotten. After the crash caused by the falls near us had ceased, we could still hear the terrible sounds of heavy falls in the more distant recesses of the mountains.

We looked up with dismay at the cliffs over head, expecting that the next shock would detach some ruins from them. Had they fallen, we could not have escaped, as the fragments from the summit would have flown over our heads, and we should have been buried by those from the middle.

Providentially there were no more shocks that night. This earthquake was smartly felt in all parts of the mountains, as well as in the plains of the north-west provinces of Hindustan.

In the morning we removed to the left bank of the river, where there is a bed of sand of about 150 yards wide; there is a flat of soil with trees of about 20 yards wide, and immediately above it are precipices with snow on them; here we were much more secure. In the afternoon indeed, the effects of the snow melting, often caused pieces of rock to fall from above, too near our station, but we could avoid them by running over the sand to the river side, which could not be done on the right bank; besides only comparatively small pieces fell here, and in day light, so that this is much the best side to encamp on. We had the curiosity to measure trigonometrically the height of the cliff, at the foot of which we were during the shock, and found it to be 2745 feet.

This day, the 27th, we had a slight shock of an earthquake, as well as on the 28th.

Filled a new and full length clean tube with pure mercury; immediately after filling (unboiled), it stood at 20 inches.

Thermometer attached	78°
Ditto detached	68°

Having hung the barometer up in the tent, and allowed it to acquire the temperature of the air and adjusted zero, the following heights were observed :

Thermometer attached	77½°
Ditto detached	63°
Upper surface of the mercury	20 in.
Second reading an hour afterwards, mercury upper convex surface	20
An hour afterwards upper convex	20
Afternoon, outside of the tent three hours after filling the tube ; mean at four o'clock	20

There were very few and but small (air) bubbles in the column, and the vacuum was evidently pretty good, as shown by the smart cracking of the mercury against the top of the tube.

Water boils at	196°

We now begin to boil the mercury in the tube. The tube as usual broke. None but a professed artist can expect to succeed in this difficult business once in 10 times. With the unboiled mercury, there must be an error, but it should not, I think, affect the heights more than 200 feet, and generally not 100 feet; and as under the present circumstances we *cannot do* more, we must be content with such approximate altitudes ; and I reckon it of some consequence to have the heights of these places even within 200 feet, as *hitherto no idea* could be formed on the subject.

When a tube is filled with unboiled mercury, which of course contains air, it stands at first *higher* than it ought from the air dilating the column ; but, after a short time, much of the air escapes into the upper part of the tube, where the vacuum ought to be, and there expanding presses *down* the mercury in the tube, thus making it *lower* than it should be. The mean height will not differ much, perhaps not more than two-tenths of an inch in moderate heats from that shown by a boiled tube.

The barometers I had were two out of six sent from England to the Surveyor-General's Office. They were made by Berge, and are very fine instruments ; but so little attention had been paid to their packing, that the tubes of them all were found to be broken when they arrived in Calcutta, as well as most of the thermometers belonging to them. There were spare, but unfilled tubes sent with them, and some of these would not fit.

Whenever barometers are sent, there should be to each at least six spare tubes *filled in England* by the maker, and hermetically sealed; and these should be carefully packed in separate cases of copper or wood lined with flannel, and the scale *downwards* should go to 13 inches. The scale of these barometers only reaches to 19 inches. In instruments intended for India, *solidity* should be considered; we want those which will do their work *effectually*, and are not anxious that they should be *small* and easily *portable*, as we can always here find means of carrying them. The mean height of the column by such observations as I thought most to be depended on is 20 in. 837′; the temperatures of the air and mercury being 73° and 65°. From which the height of Gangautri above the sea, is, calculated by

	Feet.
M. Raymond's method..........	10319·4
Dr. Hutton's method	10306·6

Latitude observed May 27 *and* 28, 1817.

By me, reflecting circle, alternate faces, mean by A and B. Libra......................	30° 59′ 29″
Large sextant by Berge. Lieut. Herbert, four sets ditto............................	35·5
By me, reflecting circle, eight circummeridional altitudes of spica, being 24 indexes, on alternate faces	27·1
Mean latitude of Gangautri................	30·59·30·5

These were good observations, and refraction is allowed on the altitudes, according to the barometer and thermometer; and all other corrections for precession, aberration, nutation, &c. are applied as usual.

The pole star could not be seen on account of the height of the cliffs, nor any star to the south lower than those observed. The same cause most unfortunately prevented our being able to observe any eclipses of Jupiter's satellites here, or the occultation of the star ♎ libra by the moon, and I was sorry to find that my chronometers could not be depended on to show the difference of longitude in time. Though they are of the best kind, and hung in gimbals, no method of carriage that I had *then* adopted could prevent them feeling the effects of the short and continually repeated jerks they received from the uneven steps which the man who carried them on his back was obliged to make. Nothing, except a staff, can be conveniently carried in the hands, as they are so frequently employed in assisting the feet in difficult places.

The mean breadth of the Ganges at Gangotrī was (measured by the chain) 43 feet, depth 18 inches, and nearly the same depth at the sides as in the middle: the current very swift, and over large rounded stones. This was on the 26th May; the stream was then in one channel, but the effect of the sun in melting the snow was at that season so powerful that it was daily much augmented; and on our return to Gangotrī on the 2d of June, the depth of the main stream was two feet, and it was a few feet wider (but I did not then measure the width). Several shallow side channels had also been filled in the interval, and on the whole I estimate that the volume of water was doubled.

Though the frequency of the earthquakes made us very anxious to get out of our dangerous situation in the bed of the river, we resolved, as we had come so far, to leave no means untried to trace the stream as far as possible, and accordingly set out on the morning of the 29th of May, hoping to arrive at the head of the river in the course of the day. The two Gangotrī Brahmins could not give any information as to how far it might be distant; they had never been higher than Gangotrī, and assured us that no persons ever went further, except the Mūnshī, who appears by the account in the Asiatic Researches to have gone about two miles.

Mr. James Frazer visited Gangotrī in 1815, and was the first European who did so.

From Gangotrī forward up the Ganges.

Pass avalanche and fragments of rock newly fallen, and which cover the path.

Ascend a snow bed which covers the river; it is about 30 feet thick.

Over the snow bed, and descend to the open stream. Here a gorge of huge rocks obstructs the stream; they have all fallen from above.

N.B. The Brahmins say they never heard of any rock or place called the Cow's Mouth, or Gao Muc'h, or any thing like it either in sound or signification. We did not see or hear of any image whatever.

River flows under a snow bed; a rill of water from the snow to right. High precipices on both sides all the way.

Alternate avalanches of snow and rock recently fallen. River under an avalanche of 500 feet thick; the snow hard and frozen.

A great fall of the peaks. River bed filled with fallen rocks, and difficult to pass. The stream, a succession of cataracts. High peaks above.

Over fragments. Here the river falls out of a snow bed in a cascade of foam: ascend the great snow bed.

Strong ascent of the snow bed, which is about 100 feet thick, over the river.

Cross a torrent six feet wide and nine inches deep; it comes from a cleft in the peaks to the left. River here under a snow bed.

River turns the foot of high snowy peaks to the right; precipices quite perpendicular to the left. Rudra Himālaya peak, 97°, Finding that the head of the river must be more distant than we expected, we sent back to Gangotrī for a small tent.

High mural precipices rising immediately from the river to the left: snowy peaks to the right, their summits about 6000 feet above us.

Cross the river at some falls. We leaped from rock to rock with some difficulty. Present general line of snow about 200 feet above us. To the right, the face of the mountain has slipped.

Bhojpatra (i. e. birch) jungle to the right with some pines, but small and stunted. Great mural precipices to the left.

Begin to pass a great *snow bed* from under which the river falls in a cascade. Heavy slips of the mountain to the right.

Ascend a very steep mass of snow, which covers the river; it appears to be 300 feet thick.

Cross a rill. To the right above us are sharp snowy peaks 6000 or 7000 feet high; at their bases is some soil and loose stones, in which birch and small firs grow.

Up the rocky bed of the river, and here ascend a very *large snow bed*, which reaches from the top of the peaks to the right of the river, and conceals it: the river bed here more expanded. The feet of the mountains to the right not so steep as hitherto. To the left are precipices. Saw some musk deer among the rocks. From the top of the snow bed, a noble snowy peak (St. George) appears.

Above the left bank of the river, and by the side of the snow bed, are some birch trees and small long leaved firs, but no more cedars. This being the only convenient or safe place we could see, we halted here. The river is perceptibly diminished in bulk already, and we hope that to morrow we may see its head. The march to day was most toilsome, and rough through the loose fragments of rock which daily fall at this season from the peaks on either side to the river, in the afternoon when the sun melts the snow. Travellers should contrive to gain a safe place by noon, or they may be dashed to pieces.

It was very cold at this place, and froze all night, but we had plenty of fire wood from the bhojpatra trees. The soil was spongy, and full of rocks. The silence of the night was several times broken by the noise of the falling of distant avalanches.

By the barometer, it appeared we were 11,160 feet above the sea. Water boiled at 193° of Fahrenheit.

A little tent, which one man carries on his back, came to us; but in this trip, we ate and slept on the ground, and were well pleased to have got so far beyond Gangotrī, hitherto the boundary of research on the Ganges. Latitude observed, 30° 58′ 59″.

The place we passed the night on is elevated above the left margin of the stream, being a sort of bank formed by the ruins of fallen peaks; but as the falls are not recent, nor the slope so steep as in most places, the birch trees and various sorts of small pines and mosses have had time to fix their roots, and afford fuel and shelter. A very long and deep snow avalanche reaches from the peaks above the left bank down to the river, and conceals it. On the opposite side of the river, the cliffs are of great height and mural, except in one place where a tremendous fall has taken place, encumbering and obstructing the bed of the river. But these ruins are so frequent, that the traveller scrambles through them with little regard, except where the freshness of the fracture of the fallen masses of rock warns him to mend his pace, and get as soon as possible out of danger.

May 30.—Birch tree, halting place, forward. Thermometer, sunrise, 32°.

Set off from the middle of the snow bed.

A torrent eight feet wide, five inches deep, joins the river. Its edges are frozen.

Cross a high avalanche of snow, which conceals the river; it is very hard frozen. The bed of the river begins to be wider; large icicles hang among the rocks.

Ford a rivulet or torrent from the left 11 feet wide. Rocky and rough. Gradual ascent.

Gradually ascending among rocks. To the left, high cliffs of granite, but not so steep as before. To the right, snowy peaks; their summits about 6000 or 7000 feet high, distant about two miles. The river bed is here about two furlongs wide, and full of stones. River certainly diminished in size; it is very rapid, its bed being an ascent. We are now above the line of vegetation of trees, and past the last firs. The birches remain, but they are only large bushes; laurels also are seen, and a sort of, I believe, lichen, which grows in the rocks. The noble three-peaked snowy mountain shines in our front, and is the grandest and most splendid object the eye of man ever beheld. As no person knows these peaks or their names, we assume the privilege of navigators, and call them St. George, St. Patrick, and St. Andrew. St. George bears 129°; St. Patrick 132° 30'.

N. B. On going further, we saw another lower peak between St. George and St. Patrick, which we called St. David, and the mountain collectively the Four Saints.

A fall of the river of 12 feet over rocks, and a succession of smaller falls. The inclination of the bed of the river is considerable; it is filled with blocks of granite, white, yellow, and red, and we saw some flint. Very difficult moving here. Great slips of the mountain to the left.

Most difficult. Over masses of rock, which have fallen from above to the stream. This station is full of peril, being a very recent slip of the whole face of the mountain to the left. The broken summits cannot be less than 4000 feet high; blocks threaten to fall, and are indeed now continually coming down. I have not seen so dangerous a slip. The ruin extends about half a mile; every person made the greatest haste to get past this horrid place. The fracture of the rocks is so fresh, that I suspect this havoc must have been caused by the earthquake of the 26th; for we heard a great crash in this direction.

Over snow for the most part. An enormously high and extensive snow bed in sight in front: it entirely conceals the river, but the stream is yet 20 feet wide.

Snow all round, and above and below, except where it has melted just here on a convenient flat between the river, and the feet of the mountains to the left. All beyond is an inclined bed of snow; so we must halt here. Call it halting place near the debouche of the Ganges.

Proceeded forward to reconnoitre, and returned.

Up the river, and along snow. Mount Moira 170°; pyramid peak, 200°.

Return to ⊙, eight to halt for the sake of fire wood.

This is an excellent and safe place; no peak can fall on us; five companies, or even a battalion, might encamp here. Sublime beyond description is the appearance of the snowy peaks now so close to us. The Four Saints are at the head of the valley of snow, and a most magnificent peak, cased in snow and shining ice, stands like a giant to the right of the valley: this we named Mount Moira. The snow valley, which hides the river, appears of great extent; to morrow will show what it is.

We experienced considerable difficulty in breathing, and that peculiar sensation which is always felt at great elevations where there is any sort of herbage, though I never experienced the like on the naked snow beds, even when higher. Mountaineers, who know nothing of the thinness of the air, attribute the faintness to the exhalations from noxious plants, and I believe they are right; for a sickening effluvium was given out by them here, as well as on the heights under the snowy peaks, which I passed over last year above the Setlej; though on the highest snow, the faintness was not complained of, but only an inability to go far without stopping to take breath.

Barometer.—The tube heated, and then gradually filled with mercury half an inch at a time, and the bubbles which were perceptible, driven out by gently beating against the places they were at.

The mercury stood at.	18·854 in.
Detached thermometer	55°
Attached ditto.	53°

Height of the place above the level of the sea 12·914 feet. Water boils at 192¼°, which, according to Mr. Kirwan's table, answers to a barometer of 19·5 inches.

We are about 150 feet above the bed of the river. By day the sun is powerful, although we are so surrounded by snow; but the peaks reflect the rays. When the sun sunk behind the mountains, it was very *cold*; at night it froze. High as we are, the clouds yet rise higher. The colour of the sky is a deep blue. What soil there is is spongy. A few birch bushes are yet seen; but a large and strong ground tree or creeper overspreads the ground somewhat in the manner of furze or brambles; and it is a curious fact that the wood of this is, we think, that of which the cases of black lead pencils are made, being of a fine brittle, yet soft red grain; and the smell is the same as of that used for the pencils, and which has hitherto been called by us cedar. I have specimens of this wood; it is called, I think, chundun; I saw it on the summit of the Chour peak, and in the snowy regions of Kunaur, but did not then examine it. It will be found probably that the pinus cedrus, or cedar of Lebanon, is the deodar (or as it is called to the westward, the kailou), and no other. Nor do our mountain cedars (24 feet in circumference) yield in size or durability to those of Lebanon. But this chundun (miscalled cedar) is not even a tree; it may be called a large creeper, growing in the manner of bushes, though it is very strong, and some of its arms are as thick as a man's thigh. Of this, and also of the great cedar (deodar), and of other pines, I will send specimens.

Latitude, mean 30° 56′ 34·5″.

Good observations.—The particulars of them, as well as of all others, I have preserved.

The *strata* of rock, where exposed, near the summits of the grand snowy peaks, were very nearly horizontal, as I observed, last year, at the summits of the peaks above the Setlej; though in lower parts of the Himālaya, the rock is generally seen deeply declined, as observed between Dangul and Sookie, as well as at Jumnotri, &c.

The colour of the high rocks on the Four Saints appeared to be of a light yellow mixed with brown or black. There being a small piece of level ground here, a primary base was measured on its longest extent; it was 319 feet; with it a longer base of 667·2 feet was obtained, favourably situated for taking the heights and distances of the peaks in front. This base, being but short, and no other to be had, great care was taken in observing the angles and elevations; and they were repeated both with a fine theodolite, and reflecting instruments (my circular instrument could not be safely brought beyond Reital). The angle of altitude of Peak St. George was 14° 07′.

Height of the peak above the sea, 22,240·6 feet.

St. Patrick, height above the station	9,471
Station above the sea	12,914
Distance 42,480 feet; and height above sea, feet	22,385

(*To be concluded in our next.*)

Article XII.

Observations on certain Substances which have been supposed to act as Acids, and as Alkalies. By R. Phillips, FRS. L & E, &c.

The first volume of the Annales de Chimie contains a memoir by M. Berthollet; the object of which is to show, that if the metals, when oxidized, perform the functions of alkalies with the acids, the same oxides also act as acids with the alkalies. Mr. Smithson (Phil. Trans. 1811,) adopting a similar opinion with respect to the action of silica upon other earths, has considered it as an acid, and has employed the term *silicate* to express its compounds: thus he says that zeolite may be regarded as a silicate of alumina and soda; and he considers the compound as bearing some analogy to alum. M. Berzelius has not only admitted that silica performs the part of an acid in certain compounds, but has attributed similar powers to alumina, and employs the term *aluminate*. The following passage from his Nouveau Système Minéralogique (p. 76), would however appear to indicate that he had not clearly determined the nature of the substances included in this class:

"Selon Ekeberg, le gahnite contient

Alumine	60·00	contenant	28·2	12	ou	6
Oxide de zinc	24·25	oxigène	4·0	2		1
Oxide de fer	9·25	comme oxidule	2·0	1		
Silica	4·75		2·2	1		

On peut considérer ce minéral de plusieurs manières. Si nous ne faisons pas attention au fer et à la silice, ce sera un *aluminias zincicus*, dans lequel l'alumine contient six fois l'oxigène de l'oxide de zinc, $Zi\,A^6$, et qui peut etre coloré par le *silicias ferrosus*. D'un autre côté il peut encore être composé d'un double aluminiate de zinc et de fer, c'est-à-dire former un *trialuminias ferroso-zincicus*, de sorte que l'alumine dans toutes ces combinaisons simples, contient trois fois autant d'oxigène que le corps avec lequel elle se trouve combinée. Dans ce cas, la composition serait $f A^3 + 2 Zi\,A^3 + A^3 S$."

Dr. Thomson, in his System of Chemistry, has also adopted the idea of the action of silica as an acid; and when the authorities by which this opinion is sanctioned are considered, it will, I am apprehensive, appear useless for me to endeavour to show, that by admitting silica and alumina to be, or to perform the functions of acids, we are in danger not only of adopting a loose system of nomenclature, but also of attributing to bodies the properties of acids, whose only claim consists in their power of combination with other bodies, and which power will, consistently with Berthollet's observation, equally entitle them to be ranked in the very opposite class of alkalies.

It will be readily granted that silica does not possess any one of the more obvious properties which characterize acids; it is inodorous, tasteless, insoluble in water, or alcohol, does not affect vegetable colours, and has no immediate action upon any alkali, earth, or metal, so as to neutralize, dissolve, or form crystalline compounds with them. On the other hand, there are cases in which it appears to act as an alkali; thus, in a finely divided state, silica is dissolved by the acids generally, and with the fluoric acid it forms a peculiar compound: it is certainly considered that the silicated fluoric acid is a compound acid, but it is to be remembered that the fluoric acid possesses acidity without being combined with silica; and moreover, when silicated fluoric acid is mixed with water, the silica is precipitated; but as this is perfectly analogous to what happens when muriate of antimony is poured into water, I think that analogies are more favourable to the alkaline, than to the acid properties of silica.

With respect to alumina, it cannot for a moment be questioned that its powers as a base are much more strongly marked than those of silica; it readily combines, when minutely divided, with almost every acid; and the formation of alum must be deemed satisfactory evidence of its saturating power with respect to acids.

Alumina, however, resembles silica in its property of combining with the alkalies, potash and soda; and it is not, I believe, generally known, that with potash it so far performs the function of an acid, as to form a crystalline compound. I have, however, procured it in crystals of considerable size, and they appeared to be efflorescent, but I have not yet subjected them to analysis; and as I am not aware that any crystalline compound of silica and potash has been formed, it must, I think, be admitted, that the acid, as well as the alkaline functions of alumina, are better defined than those of silica.

Oxide of lead is a substance which possesses the power of combining with acids and alkalies in a still more remarkable degree than alumina. When this oxide is dissolved in acetic acid, it is well known that a certain quantity saturates the acid sufficiently to prevent its action upon vegetable colours, and by evaporation we procure sugar of lead; but if this solution be boiled with an additional quantity of oxide of lead, we obtain a compound (Goulard's extract of lead) which is remarkable on two accounts. First, it is a real subsalt, and soluble in water, and there is not, that I know of, a similar instance in record. Secondly, the oxide of lead in excess acts so completely as an alkali, that Mr. South has discovered, it possesses the power of turning turmeric paper brown. Again, Mr. Faraday informs me, that by boiling the solution of muriate of zinc, as usually obtained, with an additional quantity either of the metal or the oxide, a solution is produced, which acts on turmeric paper as an alkali. There cannot, therefore, be any doubt as to the power of oxide of lead and of oxide of zinc, to perform the function of an alkaline base.

The property which oxide of lead possesses of combining with the alkalies, potash and soda, or in other words, performing the function of an acid, is as perfect as that of silica; and it resembles alumina in forming a crystalline, and consequently a definite compound with an alkaline base. M. Berthollet, in the memoir already alluded to, states that when oxide of lead is boiled with lime water, very small iridescent and transparent crystals are formed. Now this compound is the more remarkable, because it results from the combination of two bodies, which possess distinctly marked alkaline properties. Similar observations may be made with regard to oxide of zinc; it combines with ammonia, potash, soda, and lime, and therefore appears to perform the functions of an acid even more extensively than oxide of lead.

The powder of Cassius is a compound which it would be difficult to describe, on the assumption that its formation depends upon the acid nature of one, and the alkaline nature of the other constituent. In fulminating gold, the metallic oxide appears to act as an acid, for it is in combination with ammonia; but with the acids, the oxide of gold performs the function of a base, giving rise to the well-known salts of gold. Oxide of tin seems in some compounds to act as an acid; thus it combines with the alkalies, potash and soda; and it also exhibits distinctly the properties of an alkali, as far as combining with acids is to be esteemed as such. If, however, the powder of Cassius be a compound of oxide of gold and oxide of tin, as is generally allowed, what functions can be attributed to them? Do they combine as acids, as is supposed to be the case, when the fluosilicic acid is formed? or do they combine as alkalies, as when lime and oxide of lead unite? or if we consider one oxide as performing the function of an acid, and the other that of an alkali, what rule have we for assigning to each its peculiar office? These remarks might be extended to a greater length, especially if the oxides of antimony were taken into the account: these have been supposed by Berzelius to act the part of acids, and he has accordingly adopted the appellations of *antimoniates* and *antimonites*. There are many cases also in which metallic oxides combine with vegetable products, such as oxide of lead with gum, starch, &c.: now in these cases, the rules of nomenclature have been so completely set at defiance, and chemical propriety so violated, as to give these compounds the appellations of gummate and amylate of lead. Are we then to consider gum and starch as acids because they combine with metallic oxides?

It would be difficult, or perhaps impossible to suggest any mode of describing such compounds, as I have adverted to, without incurring ambiguity or impropriety: I think, however, it would be possible to employ a nomenclature which would not involve the inconsistency of describing the same substance sometimes as an acid, and at others as an alkali. With this view I would propose to consider these compounds as resulting *not* from the same law as that which determines the combination of acids and alkalies, but as derived, at any rate, in most cases, from the general disposition which oxides have to combine with each other. By reverting to the original mode of expressing the compounds of silica and the alkalies, and alumina and the alkalies, we should avoid all theory, and employ terms sufficiently descriptive of the compounds.

Instead, therefore, of speaking of silicates or aluminates, we may use the term silicated or aluminated potash, soda, or lime; it may be convenient so far to regard the compounds as saline, as to consider the more distinctly marked alkaline body as the base, and without involving any theory. Thus oxide of tin possesses greater power of combination with alkaline bodies than oxide of gold does; the powder of Cassius may therefore be denominated stannated gold. Mercuriated lime, plumbated lime, antimoniated and antimonited potash, plumbated gum and zincated potash, are terms which may be employed without violating the present system of nomenclature, and without confounding bodies whose properties are not merely distinct, but diametrically opposite. The compounds of metallic oxides with ammonia might be included in this method; thus we might say cuprated or zincated ammonia; but as no ambiguity arises from the use of the term ammoniuret, it would be worse than useless to attempt any alteration in these cases.

ARTICLE XIII.

ANALYSES OF BOOKS.

An Historical and Descriptive Account of the Steam Engine, comprising a general View of the various Modes of employing Elastic Vapour as a prime Mover in Mechanics; with an Appendix of Patents and Parliamentary Papers connected with the Subject. By Charles Frederick Partington, of the London Institution. 8vo. London, 1822.

THE great importance of the steam engine in a commercial point of view will, perhaps, render it unnecessary for us to offer any thing in the way of apology for presenting our readers with a brief notice of this stupendous machine, of which a detailed account is given in the above work.

The historical data furnished by Mr. P. certainly throw considerable light upon the early history and subsequent improvements which have been effected in the steam engine, and to this part of the work we shall principally confine ourselves.

"Among the numerous competitors for the honour of having first suggested steam as a moving power in mechanics, we must certainly place Brancas and the Marquis of Worcester in the foremost rank. The former of these was an Italian philosopher, of considerable eminence, and who, in 1629, published a treatise, entitled 'La Machine,' &c. which contained a description of a machine for this purpose. The apparatus employed by Brancas was in fact nothing more than a large æolipile, similar to the blowpipe suggested by M. Pictet, of Geneva, with this difference, that the aperture in the pipe connected with the body of the æolipile instead of being directed towards the lamp (or in this case the furnace that heated the machine) was made to strike against the floats or vanes of a wheel, by which means a rotatory motion was produced."

"After the publication of this scheme, which it is probable was never put in practice with any useful effect, nearly 30 years elapsed ere the further consideration of this important subject was resumed by the Marquis of Worcester. The mode of employing steam recommended by the Marquis, and which he describes in his 'Century of Inventions' to have completely carried into effect, was entirely different from that of his predecessor; and it is evident that the noble author had received no previous hint of Brancas's invention, as he expressly states in another part of the above work, that he 'desired not to set down any other men's inventions;' and if he had in any case acted on them, 'to nominate likewise the inventor.'"*

"In 1683, a scheme for raising water by the agency of steam was offered to the notice of Louis XIV. by an ingenious English mechanic of the name of Morland. This, however, was evidently formed upon the plan previously furnished by the Marquis of Worcester in his Century of Inventions. Morland was presented to the French monarch in 1682, and in the course of the following year, his apparatus is said to have been actually exhibited at St. Germain's."

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* "This work was written about the middle of the seventeenth century, and, considered as the united discoveries of one individual, is certainly one of the most extraordinary scientific productions which has yet issued from the press in any age or nation. In addition, however, to its value, as containing the first tangible suggestion for the employment of steam as an hydraulic and pneumatic force, it has unquestionably formed the foundation of a large portion of patent inventions which make so prominent a feature in the present day. The praiseworthy labours, however, of this indefatigable nobleman shared the fate which usually attends on projectors; and it was left to the slow though certain march of scientific improvement to award to his memory a posthumous praise. The Marquis also published a work, entitled "An Exact and True Definition of the most Stupendous Water-commanding Engine, invented by the Right Hon. (and deservedly to be praised and admired) Edward Somerset, Lord Marquis of Worcester, and by his lordship himself presented to his Most Excellent Majesty Charles II. our most gracious Sovereign." This was published in a small quarto volume of only 22 pages, and consists of little more than an enumeration of the wonderful properties of the above engine; and it is most probable that he never published any key to the first hint furnished in the Century of Inventions."

The claim lately made by the Americans to the invention of the steam boat is completely set at rest by reference to Mr. P.'s work, in which we find, under the head of Steam Navigation, p. 53, the following curious historical data:

"In 1698 Savery recommended the use of paddle wheels similar to those now so generally employed in steam vessels, though without, in the remotest degree, alluding to his engine as a prime mover; and it is probable that he intended to employ the force of men or animals working at a winch for that purpose. About 40 years after the publication of this mode of propelling vessels, Mr. Jonathan Hulls obtained a patent for a vessel in which the paddle wheels were driven by an atmospheric engine of considerable power. In describing his mode of producing a force sufficient for towing of vessels and other purposes, the ingenious patentee says: 'In some convenient part of the tow boat, there is placed a vessel about two-thirds full of water, with the top close shut. This vessel being kept boiling, rarefies the water into steam: this steam being conveyed through a large pipe into a cylindrical vessel, and there condensed, makes a vacuum, which causes the weight of the atmosphere to press on this vessel, and so forces down a piston that is fitted into this cylindrical vessel in the same manner as in Mr. Newcomen's engine, with which he raises water by fire.

"Mr. Hull's patent is dated 1736, and he employed a crank to produce the rotatory motion of his paddle wheels; and this ingenious mode of converting a reciprocating into a rotatory motion was afterwards recommended by the Abbé Arnal, Canon of Alais, in Languedoc, who, in 1781, proposed the crank for the purpose of turning paddle wheels in the navigation of lighters."

Mr. Partington gives the following account of the improvements effected by Mr. Watt:

"Mr. Watt's attention was first drawn to this subject by an examination of a small model of an atmospheric engine belonging to the University of Glasgow, which he had undertaken to repair. In the course of his experiments with it, he found the quantity of fuel and injection water it required, much greater in proportion than in the larger engines; and it occurred to him that this must be owing to the cylinder of this small model exposing a greater surface in proportion to its contents, than was effected by larger cylinders. This he endeavoured to remedy, by employing non-conducting substances for those parts of the engine which came in immediate contact with the steam. After a variety of experiments, the results of which we shall presently describe, he succeeded in constructing a working model, capable of producing a force equal to 14 pounds on every inch of the piston, and which did not require more than one-third of the steam used in the common atmospheric engine to produce the same effect.

"It will be evident that this was as near an approximation towards perfection as could possibly have been expected; and indeed much more than was likely to be effected in a large engine, as the vapour left beneath the piston possessed only 1-15th part of the elastic force of the steam employed to form the vacuum.

"Having discovered that the great waste of caloric in the old engine, arose from the alternate heating and cooling the cylinder, by the admission and subsequent condensation of the heated steam, Mr. Watt perceived that to make an engine in which the destruction of steam should be the least possible, and the vacuum the most perfect, it was necessary that the cylinder should remain uniformly at the boiling point; while the water forming the steam was cooled down to the temperature of the atmosphere. To effect this, he employed a separate condensing vessel, between which, and the hot cylinder, a communication was formed by means of a pipe and stop cock.

"To understand the action of this engine, we may employ a common syringe, connected with a boiler, as in the atmospheric engine, and furnished with a pipe passing into an air-tight vessel, immersed in water for the purpose of condensation.

"If the piston be then raised, and the communication with the condenser cut off, the steam will speedily expel the air; when this is effected, the further admission of steam must be prevented, and the communication with the condenser opened. The steam will now expand itself, passing down the pipe and entering the condenser; the moment, however, that it comes in contact with the sides of the cold vessel, it will be condensed and a vacuum formed; and this process will continue to proceed, so long as any steam remains beneath the piston.

"The only objection that offered itself to this admirable mode of condensation, arose from the difficulty experienced in getting rid of the water and air that remained in the condensing vessel. When steam was generated from water that had been freed from air by long boiling, a considerable advantage was obtained; and it was found that a power nearly equal to the entire pressure of the atmosphere was produced. The great advantage thus obtained will be sufficiently obvious, when it is known that, in the engines previously constructed, the elasticity of the steam arising from the heated injection water remaining at the bottom of the cylinder, was equal to one-eighth of the atmospherical pressure, and consequently destroyed an equal proportion of the power of the engine.

"The mode of condensing the steam, by the application of cold water to the outside of the condenser, was soon found inconvenient from the great size and expense attendant on the use of this apparatus; and Mr. Watt introduced an internal jet of cold water, which, striking against the steam, instantaneously reduced it to its original bulk, and thus formed a vacuum. To draw off the condensing water, as well as to get rid of the air that was extricated during condensation, he found it necessary to employ a small pump, worked by the engine, the size of which was proportioned to the amount of air and water generated in the condenser. In one of the early engines upon this construction, erected at Bedworth, three air-pumps were used; two below, worked by chains connected with the beam, and a third, placed above, which received the hot water raised by the others. In the engines now constructed, only one air-pump is employed, and this fully answers the intended purpose.

"Another improvement introduced by Mr. Watt, consisted in surrounding the upper part of the cylinder with a cap, through a hole in the centre of which the piston rod worked air-tight. The force of steam was then substituted for that of the atmosphere, and at a pressure of more than fifteen pounds on the square inch; so that when a vacuum was formed beneath the piston, steam of considerable impellent force was entering the upper end of the cylinder, by means of a pipe connected with a boiler.

"By thus substituting the force of highly elastic vapour, for the ordinary pressure of the atmosphere, the upper and under side of the piston were preserved at the same temperature, and the supply of steam being regulated by the width of the aperture, any given amount of force might readily be produced. In the atmospheric engine this could not be effected, as the whole pressure of the atmosphere was made to act on the piston, the instant the vacuum was formed by the condensation of the vapour beneath; so that in the event of a pump-rod breaking, by which the elevation of the water might be impeded, and the labour of the engine taken off, the rapid descent of the piston would evidently cause the destruction of the entire apparatus.

"Soon after the completion of his first model, Mr. Watt erected an engine for his friend Dr. Roebuck of Kinneil, near Borrowstownness, with whom he was afterwards associated in the manufacture of his improved engine: the latter gentleman, however, in 1774, disposed of his share of the business to Mr. Boulton, of Soho."

Want of room prevents our making any additional extracts from Mr. P.'s work, or attempting an enumeration of the various engines he describes, which could only be satisfactorily accomplished by reference to the numerous plates employed for their illustration.—But it may be adviseable before we finally dismiss the subject, to briefly notice another work of a more general nature, but with much higher pretensions, announced as far back as 1816, though but just published. We allude to the new edition of Professor Robison's Mechanical Philosophy, edited by Dr. Brewster. The article Steam Engine, after having been revised by the late Mr. Watt and the learned editor, has been put forth by Mr. Murray, as "the only account of the steam engine that can be relied upon." What claims it possesses to this title, may easily be seen by reference to a very simple fact. The last steam engine described in Professor Robison's Mechanical Philosophy, was erected for the Albion Mills, in 1788, since which period we find, by turning to Mr. Partington's appendix, that more than one hundred patents have been enrolled, many of which are of the utmost importance.

Transactions of the Cambridge Philosophical Society, Vol. I. Part II. 1822.

From an accidental cause, we omitted to notice the first part of this Society's Transactions; we, therefore, take an early opportunity of giving a brief sketch of the contents of the present part.

I. *Analysis of a Native Phosphate of Copper from the Rhine.* By F. Lunn, Esq.

As this paper has been given entire in the *Annals*, it is unnecessary to notice it upon the present occasion.

II. *Upon the regular Crystallization of Water, and upon the Form of its primary Crystals.* By Dr. E. D. Clarke.

This communication of the late and lamented Professor is accompanied by a plate, which is indeed requisite to the perfect understanding of his views.

After mentioning various authors who have treated on the same subject, and described the appearances which crystallized water assumes, Dr. Clarke concludes his memoir with observing: "It is presumed, therefore, that the question respecting the crystallization of water may be set at rest by these phenomena; because it is now no longer a mere inference deducible from observing the intersection and disposition of the spiculæ exhibited by water when frozen upon the surfaces of other bodies, and in its approach to crystallization; but it is a decided fact, shown by regular crystals of ice, that the compound we call water, or hydrogen oxide, crystallizes both in hexahedral prisms and in rhombi, having angles of 120° and 60°; and that the latter is its primary form. The manner too in which these forms have been displayed may guide to the crystalline forms of other bodies, by inducing a careful examination of the surfaces, points, and interstices of all minerals when they are found as stalactites. The stalactite formation is of all others the most likely formation *to bear marks of a regular* crystallization; because it is the result of a process in which the particles of bodies are not carried by a too sudden transition from the fluid to the solid state; but gradually approach, and become united by virtue of their mutual attractions, as the moleculæ of the fluid which had separated them go off by evaporation or by other causes. And in further confirmation of this, it may be urged, that when the crystallization of the stalactite carbonate of lime, and of other stalactites, especially chalcedony, had been considered as impossible formations, contradictory to the laws by which Nature acts in the stalactite process, yet the primary form of the carbonate of lime is nevertheless exhibited by the stalactites of the cavern of Antiparos, and the primary form of the hydrates of silica by the stalactites of blue chalcedony brought from the Hungarian mines."

III. *On the Application of Hydrogen Gas to produce a moving Power in Machinery; with a Description of an Engine which is moved by the Pressure of the Atmosphere upon a Vacuum caused by Explosions of Hydrogen Gas and Atmospheric Air.* By the Rev. W. Cecil, MA. Fellow of Magdalen College, and of the Cambridge Philosophical Society.

The author of this paper observes that "two of the principal moving forces employed in the arts are water and steam. Water has the singular advantage that it can be made to act at any moment of time without preparation; but can only be used where it is naturally abundant. A steam-engine, on the contrary, may be constructed at greater or less expense, in almost any place; but the convenience of it is much diminished by the tedious and laborious preparation which is necessary to bring it into action. A small steam-engine, not exceeding the power of one man, cannot be brought into action in less than half an hour; and a four horse steam engine cannot be used under two hours' preparation."

The engine in which hydrogen gas is employed to produce moving forces was intended to unite the two principal advantages of water and steam so as to be capable of acting in any place without the delay and labour of preparation.

The general principle of this engine, as described by Mr. Cecil, is founded upon the property which hydrogen gas, mixed with atmospheric air possesses, of exploding upon ignition, so as to produce a large imperfect vacuum. If two and a half measures of atmospheric air be mixed with one measure of hydrogen, and a flame be applied, the mixed gas will expand into a space rather greater than three times its original bulk. The products of the explosion are a globule of water, formed by the union of the hydrogen with the oxygen of the atmospheric air, and a quantity of azote, which in its natural state (or density 1) constituted ·556 of the bulk of the mixed gas; the same quantity of azote is now expanded into a space somewhat greater than three times the original bulk of the mixed gas; *that is, into about* six times the space which it before occupied;

its density is, therefore, about one-sixth, that of the atmosphere being unity.

According to Mr. Cecil, if the external air be prevented by a proper apparatus from returning into this imperfect vacuum, the pressure of the atmosphere may be employed as a moving force, nearly in the same manner as in the common steam engine; the difference consisting chiefly in the manner of forming the vacuum.

Mr. Cecil then enters into an estimate of the power resulting from such a vacuum by comparing the effects of equal bulks of steam and hydrogen; this it will be impossible to comprehend without the diagram by which it is illustrated; but the author concludes, that "it appears by calculation that any quantity of pure hydrogen gas will produce more than five times the effect of the same bulk of steam; and in practice the disproportion of their effects is still greater. It is here supposed, that steam produces by condensation a perfect vacuum equal to its own bulk; but this is far from being the case: much of the power is lost by needless condensation by the escape of steam through the piston, besides a considerable deduction for working an air pump, and two water pumps, which are necessary to a steam engine.

This paper is accompanied with a drawing and explanation of a model of a gas engine. The drawings are adapted to the Isometrical Perspective of Prof. Farish. There is also a drawing of one of a different construction which Mr. Cecil has introduced on account of its simplicity.

The paper concludes with some observations upon the use of the explosive force of gunpowder as a moving force, and with showing that it cannot be practically useful, for several reasons, and particularly from the corrosion of metals by the sulphur contained in the gunpowder, and by the sulphuric acid which is produced during combustion.

IV. *On a remarkable Peculiarity in the Law of the extraordinary Refraction of differently-coloured Rays exhibited by certain Varieties of Apophyllite.* By J. F. W. Herschel, Esq. FRS. of London, Edinburgh, and Gottingen, &c. &c.

In this paper Mr. Herschel refers to the figures contained in the first part of the Transactions; and as without these, it would be imperfectly intelligible, we shall not attempt any analysis of this paper.

(To be continued.)

Article XIV.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

June 6. On the Binomial Theorem, by John Walsh, Esq.

A paper, by Dr. Davy, was likewise read, entitled "Some Observations on Corrosive Sublimate." It is known that the liquor hydrargyri oxymuriatis of the London Pharmacopœia, on exposure to light, slowly undergoes decomposition; and it has been asserted that light has a similar effect on corrosive sublimate itself. Dr. Davy relates a number of experiments made to investigate these points. He finds that corrosive sublimate remains unaltered on exposure to light; that it remains unaltered when exposed in solution in media, having a strong affinity for it, as alcohol, ether, muriatic acid, &c. and that decomposition takes place only under circumstances of complicated affinities, as in the instance of the liquor hydrargyri oxymuriatis, and in the aqueous solution, when calomel and muriatic acid appear to be formed, and oxygen evolved.

For the purpose of further illustration of the subject, Dr. Davy describes a series of experiments on corrosive sublimate with alcohol, ether, several oils, muriatic, and the mineral acids, many of the muriates, &c. the results of which hardly admit of being given in the form of abstract. In every instance that an oil, whether volatile or fixed, was heated with corrosive sublimate, mutual decomposition took place, charcoal was evolved, and muriatic acid and calomel formed. Besides, when oil of turpentine was used, some traces of artificial camphor appeared; and when the oils of cloves and peppermint, a purple compound distilled over, consisting of the oil employed, and muriatic acid. With muriatic acid, common salt, and some other muriates, corrosive sublimate formed definite compounds remarkable for their solubility.

June 13.—On the State of Water and Aeriform Matter in the Cavities of certain Crystals, by Sir Humphry Davy, Bart. PRS.

June 20.—Some Experiments on the Changes which take Place in the fixed Principles of the Egg during Incubation, by W. Prout, MD. FRS.

The author, after a few preliminary remarks, proceeded to relate his experiments on the recent egg. The specific gravity of new laid eggs was found to vary from 1080 to 1090. Eggs, however, as is well known, on being kept for some time, become specifically lighter than water, owing to the substitution of air for a portion of their water which escapes. Thus it was stated that an egg exposed for two years to ordinary circumstances, lost nearly two-thirds of its weight. Experiments were next related, the object of which was to attempt to ascertain the relative weights of the shell, albumen, and yolk. For this purpose the eggs were boiled hard in distilled water, and the different parts weighed in their *moist* state. The average of 10 experiments gave for the shell 106·9, albumen 604·2, and yolk 288·9, on the supposition that each egg originally weighed 1000 grs. to which standard the weights of all the eggs were reduced. These experiments show that the relative weights of these different portions of the egg differ very considerably, particularly the shells, the weights of which were found to vary from 77·6 to 108, on the supposition that the original weights of the two eggs were equal. An egg, when boiled, and cooled in the air, always lost considerably in weight; and the water was found to contain traces of most of the saline contents of the egg.

After these remarks on the recent egg, the author proceeded to relate the results of his analysis of the egg at the end of the first, second, and third week of incubation, and arrived at conclusions of which the following may be considered as an outline:

1. That an egg loses about one-sixth of its weight during incubation—a quantity amounting to eight times as much as it loses in the same time under ordinary circumstances.

2. That in the earlier stages of incubation, an interchange of principles apparently takes place between the yolk and a portion of the albumen; that this interchange is confined on the part of the yolk to a portion of its oily matter, which is found mixed with a portion of the above-mentioned albumen. That this portion of albumen undergoes some remarkable changes, and is converted into a substance analogous in its appearance, as well as some of its properties, to the curd of milk; and, lastly, that a portion of the watery parts of the albumen is found mixed with the yolk, which becomes thus apparently increased in size.

3. That as incubation proceeds, the saline and watery matters again appear to quit the yolk, which is thus reduced to its original bulk, or even becomes less than natural; and that in the last week of the process, the greater portion of the phosphorus quits the yolk likewise, and is found chiefly in the animal, where it exists as phosphoric acid, and in union with *lime*, constituting its bony skeleton, *which lime amounting to about three grains, does not pre-exist in the recent egg, but makes its appearance, in some unaccountable manner, during the process.*

The author then proceeded to make a few remarks on the source of the earthy matter, which, he observed, must be either derived from the shell, or from the transmutation of other principles. The great difference existing among the shells of different eggs rendered it impossible to determine by chemical means, and the application of averages, whether it was derived from the shell or not; but the extravascular position of the earthy matter of the shell, the separation of the membrana putaminis in the latter stages of incubation, and particularly the singular fact of the small quantity of earthy matter, originally existing in the egg, remaining unappropriated at the end of the process of incubation, rendered this opinion very improbable. The author, however, left this point to be determined by future observation.

GEOLOGICAL SOCIETY.

April 19.—On the Formation of Vallies by Diluvial Excavation, as illustrated by the Vallies which intersect the Coast of Devon and Dorset. By the Rev. W. Buckland, F.R.S. F.L.S. V.P.G.S. and Prof. of Geology and Mineralogy, University, Oxford.

The author, on presenting the society with two sectional views of the coast on the east of Lyme, and on the east of Sidmouth, is led to consider the general causes to which vallies owe their origin, and particularly such as occur in horizontal and undisturbed strata within the limits of their escarpments.

Many vallies may be ascribed to the elevation or depression of the strata composing the adjacent hills, by forces acting at very remote periods from within the body of the earth itself; and to similar forces, principally we may refer the high inclinations and contortions of the strata that compose the most elevated mountains, and some also of the minor hills.

Other vallies have been occasioned by the strata having been originally deposited at irregular levels, and others to some partial slips or dislocations of portions of strata.

But at different periods of time, intermediate between the deposition of the most ancient and the most recent of the strata, the irregularities of level arising from the preceding causes, have been variously modified by the action of violent inundations, hollowing out portions of the surface, and removing the fragments to a distance. To such inundations, we must ascribe the water-worn pebbles of the red marl and of the plastic clay formations.

A cause similar to that last mentioned, has wrought extensive changes on the surface, however variously modified by preceding catastrophes, at a period subsequent to the deposition and consolidation of the most recent of the regular strata. For rocks of all ages bear on those portions of their surface which are not covered by more recent strata, the marks of aqueous excavation, and are strewed over with the mingled fragments of the most recent, as well as of the most ancient beds.

When one or more sides of a valley are formed by any of those abrupt escarpments, such as usually terminate the outgoings of our secondary strata, it is then difficult to say to what extent the discontinuity of the strata and the formation of the valley, beyond the limits of the escarpments are attributable to the last of the above recited causes; for we know not how far the strata originally extended beyond their present frontier, nor how much of the subjacent valley is referable to other causes than the most recent diluvian agency. But when a valley occurs within the limits of the escarpment of strata, which are horizontal, or nearly so, and which bear no marks of having been moved from their original position, by elevation, depression, or disturbance of any kind; and when such valley is inclosed along its whole course by hills that afford an exact correspondence of opposite parts, it must be referred exclusively to the removal of the substance once filling it, and the cause of that removal appears to have been a violent and transient inundation. The author contends, that vallies, such as those last described, cannot have been formed in any conceivable duration of years, by the rivers now flowing through them, since each individual stream owes its existence to the prior existence of the valley through which it flows. But for further proofs and illustrations of the diluvian theory, he refers to the works of Catcott and Dr. Richardson, and of Mr. Greenough.

Of the same nature with those last described, are the vallies which form the principal subject of the present communication. Their main direction is from north to south, at right angles to the coast, and nearly in the direction of the dip of the strata in which they are excavated. The streams that flow through them are short and inconsiderable, and incompetent, even when flooded, to move any thing more weighty than mud and light sand.

The greater number of these vallies, and of the hills that bound them, are within the limits of the escarpment of the green sand formation, and in their continuation southwards cut down into oolite, lias, or red marl, according as this or that formation, constitutes the substratum over which the green sand originally extended.

There is usually an exact correspondence in the structure of the hills inclosing each valley, so that whatever stratum is found on one side, the same is discoverable on the other side, upon the prolongation of its plane. Whenever there is a want of correspondence in the strata on the opposite sides of a valley, this is referable to a change in the substrata, upon which the excavating waters had to exert their force. The section of the hills presents in general an insulated cap of chalk, or a bed of angular and unrolled chalk-flints, reposing on a broader bed of green sand; this, again, reposes on a still broader base of oolite, lias, or red marl. With the exception of the very local depression of the chalk, and the subjacent strata on the west of the Axe at Beer Cliffs, the position of the strata is regular and slightly inclined, nor have any subterraneous disturbances operated to any important degree, to affect the form of the vallies.

The mass of chalk which at Beer Head composes the entire thickness of the cliff, gradually rises westwards, with a continual diminution of its upper surface, until after becoming more and more thin it finds in Dunscombe hill its western boundary. Beyond this boundary, on the top of all the highest table lands and insulated summits, from the ridges that encircle the vales of Sidmouth and Honiton, to the summits of Blackdown and even Haldon west of the Exe, angular chalk flints are found. Similar chalk flints are found on the summits of green sand that encircle the vallies of Charmouth and Axminster; and large insulated masses of chalk itself are found along the coast from Lyme, nearly to Sidmouth, and in the interior at Wideworthy, Membury, Whitestanton, and Chard, at the distances of from 10 to 30 miles from the escarpment of the chalk. These facts concur to show, that there was a time when the chalk covered all those spaces on which the flints are now found, and that it probably formed a continuous stratum, from its present termination in Dorsetshire, to Haldon west of Exeter.

Similar observations are made by the author concerning the green sand, and similar inferences are drawn from them as to the former continuity and subsequent excavation of its strata.

May 3.—A Paper was read, entitled "Additional Notices on the Fossil Genera Icthyosaurus and Plesiosaurus," by the Rev. W. Conybeare, M.G.S.

This paper consists principally of anatomical details not susceptible of abridgment. It fills up the outline of the history of the fossil genera Icthyosaurus and Plesiosaurus, sketched in a preceding communication published in the 5th vol. Transactions Geological Society, and establishes five different species of Icthyosaurus, principally distinguished by the form of their teeth. A particular account of the dentition of this genus is given by the author, from which it appears that it resembles that of the crocodile in the general form of the teeth, and the general mode in which the secondary teeth replace the first set; but differs in the circumstance, that the latter teeth become in advanced age, completely solid, by the ossification of the pulpy matter filling the interior cavity, which in the crocodile always remains hollow, a constant developement of successive series of new teeth taking place in the latter animal. In this point the dentition of the Icthyosaurus agrees with the other genera of the Saurian order, to which the term lacertian may most strictly be applied.

The comparative analogies of structure exhibited by the Icthyosaurus to both these branches of the Saurian order, are examined and illustrated in detail in the present communication, but the author hesitates to pronounce any decisive opinion as to the question, whether it approximates most nearly to the former or the latter class; considering its characters as in many respects intermediate, and the combination of those characters as constituting a whole entirely sui generis.

In the course of this detail, the structure of the temporal fossæ, the parts surrounding the meatus auditorius, the posterior bones of the head, and the palatal and pterygoidal parts of the roof of the mouth, are minutely investigated.

Of the new Saurian genus Plesiosaurus (the discovery of which is due to the present author), the bones of the head which had not been discovered when the former communication was published, have since been procured. The teeth in this genus are placed in distinct alveoli, and in all respects resemble those of the crocodile; but in almost every other respect, the analogies presented by the head of this animal are much more closely allied with the lacertian genera.

The nostrils are small, and placed as in the Icthyosaurus; so that the olfactory organs must have been much less developed than in any recent Saurians.

The comparative shortness of the snout in the Plesiosaurus gives to the whole head a general character entirely dissimilar to that of the Icthyosaurus, yet many of its separate parts offer strong analogies with this genus also.

May 17.—Notice on a Fossil Bone found in the neighbourhood of Cuckfield, Surrey, by Capt. Vetch, MGS.

The bone mentioned in this notice was obtained from a bed of ferruginous sandstone, a short way north of Cuckfield in Sussex; this bed is 6 feet thick, resting upon blue clay, about 3 feet from the surface; and within the sandstone is a bed of limestone, about a foot thick; and the bone, under examination, was found at the upper junction of the limestone and sandstone partly imbedded in both. The bed of sandstone varies considerably in its thickness and dip; and the beds of limestone which it contains also vary in thickness and number. These two rocks contain vegetable remains, shells, and numerous small fragments of bone. That under notice is, however, of considerable size, but was evidently at the period of its envelopement in the sandstone, very imperfect.

The fact of the bones in this bed being so much broken and dispersed, would seem to show that they had been subjected to the action of some considerable force, probably of water; and as the fragments have not the appearance of being water worn, it may have been, that the bed of sandstone is not their original repository, but they had been lodged in a previous bed of sand or mud, till so far decayed as to be easily broken by slight forces.

From the appearance and internal structure of the bone under consideration, it may, the author conceives, be inferred, that it belonged to an aquatic animal, and if compared with the osteology of the whale, it bears some resemblance to the jaw of a small subject of that tribe, and still more to the rib of a large one. It is not improbable, however, that it has belonged to a genus very distinct from any we are now acquainted with. A smaller bone, procured near the same place, resembles part of the spine of a large animal, and may have belonged to the bone of the same individual with that in question.

Observations on the Strata of Tilgate Forest, in Sussex. By Gideon Mantell, Esq. MGS.

This paper is an abstract of a more detailed account which has, since the last meeting of the Society, been published in the author's work on the Geology of Sussex, and is intended merely to illustrate a series of specimens now presented by him to the Society.

Notice on the Stonesfield Slate Pits. By Henry Hakewill, Esq. MGS.

The quarries from whence the specimens referred to in this communication were obtained, are in the village of Stonesfield, situated about three miles north-west from Woodstock, in Oxfordshire, on the north bank of the valley, in which the river Evenlode runs, and at a considerable elevation above the river. The strata from which the Stonesfield *slates* are made, occur at about 60 feet from the surface of the earth, and are worked by means of shafts sunk to that level, and the *vein* (as the bed sought after is called) is followed in an excavated gallery: the pendle, which is the name given to the bed from which the slates are made, consists of two distinct strata, separated by a gravelly vein of about a foot and a half thick called *race;* the upper course of the slatestone is about 10 inches in thickness, with excrescences of a circular form attached to it, called by the workmen bolt downs, or whims.

The lower stratum of the pendle is one foot thick, and upon its upper surface are excrescences of a similar form, called caps. In the race are found numerous spherical nodules, flattened at the sides, six inches to four feet in diameter, but most commonly about two feet. Immediately above the pendle, there is occasionally a coarse stone, and in the pendle itself are found those interesting remains of animals, which have drawn the attention of geologists to this spot.

The slates are made from the stone dug in the summer, and brought to the surface, and spread out with the grain exposed to the weather; and, during the winter, it is frequently watered; the frost assists materially in dividing it into slates.

June 7.—A letter was read, accompanying specimens from Dr. Wallich of the Residency of Napal.

These specimens were brought from Mucktinath, a place at a distance of about 20 days' journey north-west from the valley of *Napal,* and probably at a very considerable elevation above it.

They are said to occur always in the form of rolled pebbles, and to constitute almost entirely the bed of the river called Salagrami. The specimens themselves are of that sort which the Hindoos worship under the name of Salagrams; the present kind being called Shesha Kundala. They consist of a very firm variety of a blackish argillaceous rock, and their form is that of ammonites in which they seem to have been moulded.

ARTICLE XV.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Definition of a Straight Line.*

A correspondent states that he shall feel obliged by any objection to the following definition of a straight line:

A straight line is such as being divided or produced to any extent, is still directed towards the same points.

II. *Black Urine.*

It appears from Dr. Marcet's paper in the Medico-Chirurgical Transactions, that he has met with some cases in which black urine had been voided. At the request of Dr. Marcet, some was examined by Dr. Prout, who gives the following account of its chemical properties:

The residuum obtained from this urine by evaporation not only does not contain any lithic acid, as was observed by Dr. Marcet, but no urea can be detected in it by the tests which indicates its presence.

Although the addition of dilute acids produced no immediate change of colour in the urine, yet, on standing for some time, a black precipitate slowly subsided, leaving the supernatant fluid transparent, and but slightly coloured.

The black precipitate thus obtained was found to be nearly insoluble either in water or alcohol, whether hot or cold. It readily dissolved in cold concentrated sulphuric and nitric acid, forming a deep brownish-black solution; but, on diluting the acids with water, the black substance appeared to be again precipitated unaltered. These acids, however, by the assistance of heat, apparently decomposed it. The black substance readily dissolved in the fixed alkalies and in the alkaline subcarbonates, forming very dark solutions. The addition of water did not affect these solutions; but acids re-precipitated the substance apparently unchanged. When ammonia was employed as the solvent, and the excess expelled by evaporation to dryness, a black or deep brown residuum was obtained, which appeared to be a compound of the black substance with ammonia, and possessed the following properties:

It was very soluble in water; and, on being heated with caustic potash, it gave off the smell of ammonia. The black compound, however, did not appear to have any tendency to assume the crystalline form.

In evaporating to dryness, on a piece of glass, the ammoniacal solution in which the black substance had been dissolved, the residuum split into most minute fragments, having a regular and very peculiar appearance, especially when examined with a magnifier.

From the solutions of this compound in water, muriate of barytes and nitrate of silver produced copious brown precipitates, as did also protonitrate of mercury and nitrate of lead; but oxymuriate of mercury produced no immediate precipitate, and that obtained from acetate of zinc was of a paler brown colour.

From these experiments Dr. Prout concludes that the remarkable specimen of urine in question owes its black colour to a compound of a peculiar principle with ammonia, as Dr. Marcet had inferred from his own trials; but he is moreover inclined to think that the black principle itself, such as obtained from the urine by the action of dilute acids, may be considered as a new body possessed of acid properties. From the small quantity of the specimen, however, which could be spared for Dr. Prout's experiments, it was impossible to obtain complete and decisive evidence on the nature of this substance; but it appears to be sufficiently characterized as a peculiar acid, and to bear a closer analogy to the lithic acid, or rather to some of the compounds which it forms when acted upon by the nitric acid, than to any other principle usually found in the urine.

Should this view of the subject be confirmed by farther observations, Dr. Prout would propose to distinguish this new substance, on account of its black colour by the name of *Melanic acid.*

III. *Details of a remarkable Phenomenon, which occurred in the Commune of Juvinas, June 15,* 1821.

The following extract from the Register of the Civil Department of Juvinas appears worthy of notice, not merely from the fact which is there recorded, though it is curious, but from the nature of the recital. It is astonishing, that in the 19th century the narration of a well-known meteorological phenomenon should be accompanied by the relation of circumstances which recall the ignorance of past ages; that five hundred devils should be named as the presumed agents of the fall of an aerolite; and that to discover this stone, it was judged more proper to carry holy water than mattocks and levers.

That a proces-verbal, in which all these absurdities are recorded, should be signed by magistrates, filling important offices, is still more surprising.

The frequent fall of aerolites, during the last fifty years, has fixed the attention of naturalists to the subject. It is generally in calm weather, observes M. Leman, under a cloudless sky, that these phenomena are observed: a ball of fire is perceived, which traverses a certain space, variable in its direction, and which soon bursts with a noise resembling fire-works, or a battery of cannon at a distance. When it is extinguished, a small white cloud is seen in the same spot, which is quickly dissipated, and it falls upon the ground, sometimes, in large fragments, but more frequently in small quantities, and even single stones. In falling, the stone pierces the ground for a considerable distance, according to its size and hardness. At the time of its fall, it is hot, and gives out a sulphurous smell; it is covered entirely with a black crust, without it has struck, in its fall, against a rock or very hard substance; then it flies into a thousand pieces, and shows no crust.

With these preliminary observations, persons the least instructed in meteorological phenomena, will be able to form an exact idea of the facts contained in the following account, of which the copy is duly certified.

We, Mayor of the Commune of Juvinas, Canton d'Antraigues, Arrondissement de Privas, departement de l'Ardêche, report, that on the 15th of this present June, warned by a frightful noise, which was heard in our commune, and those which surround it: about three o'clock in the afternoon, we apprehended that some great and extraordinary event was about to effect a general destruction in nature, which obliged us successively to adopt regulations to satisfy us, that no one in our jurisdiction had been the victim of the phenomenon which at first appeared to be inexplicable.

At length, after some days had elapsed, we were informed that a meteor, of which history furnishes no similar account, had burst upon the mountain de l'Oulétte, in the hamlet of Cros du Libonez, forming a part of our commune; and, according to Delmas, who is seventy years of age, its appearance was preceded and announced by two strong explosions, occurring nearly together, resembling the discharges of two large cannons, and followed by a frightful noise, that continued for more than twenty minutes, which spread alarm and consternation amongst the inhabitants, who believed they should be immediately swallowed up by some abyss ready to open under their feet: the flocks fled, and the goats and sheep collected in groupes. At the same time a black mass was seen coming from behind the mountain de l'Oulétte, describing, as it descended in the air, a quarter of a circle, and sinking into the hollow of the valley of Libonez.

This remarkable circumstance was scarcely perceived by any but children, who, less alarmed than more competent persons would have been, followed the direction, and have since pointed out the exact spot where this mass was swallowed up. Delmas adds that he heard in the air a confusion of voices, which he thought were, at least, five hundred devils, and whom he considers as the agents that transported this alarming phenomenon: at the moment he said to Claude Vaisse, one of his neighbours (who, like himself, was in the fields) "Do you hear; do you understand the language of all these people?" This person replied frankly,—"I do not comprehend them;" but they were both persuaded that this mass was carried by infernal spirits. Delmas, for the latter reason, said to Vaisse, "we have only time for one act of contrition," cast his eyes on the ground, bowed his head, and tranquilly waited for death. Such was the consternation of all the witnesses of this terrible event that, according to their confession, they fancied they already saw the mountains rolling and heaped upon them.

The alarm was such, that it was not till the 23d of the month that they resolved to dig out this prodigy, of which they knew neither the form, the nature, or the substance. They deliberated for a long time, whether they should go armed to undertake this operation which appeared so dangerous; but Claude Serre, (sexton) justly observed, that if it was the devil, neither powder or arms would prevail against him, that holy water would be more effectual, and that he would undertake to make the evil spirit fly; after which they set themselves to work, and after having sunk nearly six feet, they found the aerolite, weighing rather more than 202 pounds (English). It was covered with a black bituminous varnish, and some parts of it had a sulphurous smell. It was requisite to break it to get it out: there still remains a mass weighing about 100 pounds.

All the facts above stated are proved by all the inhabitants of the hamlet of Libonnez; and especially Delmas, sen. and jun.; James and Claude Serre, Peter Charayre, John Chaudouard, Anthony Dumas and his child; and also by Mary Ann Vidal, a young girl of about 14 years of age; the two latter, who were less frightened, followed the direction of the stone, and actually found the place where it was buried. Concerning all which, we have drawn up the present *proces-verbal* as a continuation of the history of these phenomena, a copy of which we shall send to M. the Prefect.—(Drawn up and agreed upon at our house, the 25th of June, 1821.)

We, the Mayor of Juvinas, certify, that three days after, on the 26th of June, on visiting the place where this stone fell, another was found at a short distance from it, which weighed about two pounds and a quarter; it was covered with a similar varnish, and entirely distinct from the first. (A true copy delivered by us, the Mayor of the Commune of Juvinas, the 3d of July, 1821.) DELAIGUE.

The Master of Requests, Prefect of Ardêche, certifies that the present extract from the *proces-verbal*, written the 25th of June, 1821, by the Sieur Delaigne, Mayor of the Commune of Juvinas, agrees precisely with that which was sent officially to the prefecture, and that the fragments of the aerolite, which were brought by the Sieur Claude Fargier, are of the same nature, and present the same characters as that which has been deposited in the Museum of Ardêche.

Privas, 5th of July, 1821. TEYSONIER.

IV. *Analysis of the Aerolite which fell at Juvinas.*

M. Laugier states that he has performed four analyses of this stone, the first by means of acid, the second by potash, the third by nitric acid, with the intention of determining the quantity of sulphur; the fourth by means of nitrate of barytes, for the purpose of determining the quantity of the potash, which M. Vauquelin had found in this stone, although he did not employ this method, the only one which can be relied upon. These several analyses, all agreeing as to the nature of the elements of stone, varied slightly with respect to their proportions; a variation which must be attributed to its being deficient in homogeniety in all its parts.

The second analysis, that by potash, which appeared to M. Laugier to be the most correct, gave the following results:

Silica	40
Oxide of iron	23·5
Oxide of Manganese	6·5
Alumina	10·4
Lime	9·2
Chrome	1
Magnesia	0·8
Sulphur	0·5
Potash	0·2
Copper	0·1
Indispensable loss	3·0
Loss from unknown causes	4·8
	100·0

M. Laugier observes, that the loss of four or five per cent. which always occurred in his analyses, instead of the increase which, in these kinds of analyses, usually results from metals which the aerolites contain, renders it probable that in the aerolite of Juvinas, the iron and manganese exist in the state of oxides. No portion of this aerolite reduced to powder was attracted by the magnet, which renders this conjecture more probable.

M. Laugier endeavoured to discover whether this loss was owing to carbonic acid, but the stone did not appear to contain any: in a subsequent analysis he found, however, that it yielded rather more sulphur than stated in the analysis. He afterwards observes that this areolite resembles one which fell at Ionzac in its analysis, and especially in the absence of nickel; and also with an aerolite which fell in 1813, in the environs of Lantola, a village in the government of Wibourg, in Finland. These are the only areolites which have been hitherto found destitute of nickel.

V. *Magnesian Minerals of Hoboken.*

It appears from the observation of Mr. Nuttall, that magnesian earth pervades not only the mass of serpentine rock, which occurs at Hoboken, in New Jersey, but all the concomitant minerals, in a manner hitherto unexampled. Among the latter is the hydrate of magnesia, which contains 30 per cent. of water,* and a minute proportion of iron, the latter ingredient being found even in the purest specimens which are perfectly colourless and diaphanous. Contiguous to this is found a species of magnesian marble, forming a continuation of the same veins which afford the magnesian hydrate. It contains in 100 parts 44 magnesia, 50 carbonic acid, and variable proportions of lime, silica, and protoxide of iron.

In veins of the same rock a mineral occurs, which, from its silky lustre, and flexible fibrous texture, was at first mistaken for amianthus. It was found, however, to dissolve entirely without effervescence in acids; and, in fact, to be a hydrate of magnesia constituted of the same proportions as the foliated variety, with about five per cent of protoxide of iron. In other veins of the Hoboken serpentine, and in that of Bare Hills, near Baltimore, a mineral also has been found, which has received the name of marmolite. Its texture is foliated with the laminæ, thin, and often parallel as in diallage; its colour pale green or greenish grey; lustre pearly; soft enough to be cut with a knife, and almost perfectly opaque and inflexible. Spec. Grav. 2·470. It was found on analysis to contain

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* This, as well as the similar mineral found in Shetland, by Dr. Hibbert, appears to be the proto-hydrate, consisting of one atom of magnesia, = 18·5 + 1 atom of water = 8·5.

Magnesia	46·0
Silica	36·0
Lime	2·0
Water	15·0
Iron and Chrome	0·5
	99·5
Loss	5
	100·0

(Silliman's Journal.)

VI. *Analysis of Sulphuret of Molybdenum, found near Chester, Delaware County, Pennsylvania.* By Mr. Seybert of Philadelphia.

In internal characters it resembled so closely that of Saxony as to render any description needless. It consisted of

Sulphur	39·68
Molybdenum	59·42
Loss	0·90
	100·00

(Ibid.)

VII. *Analysis of the Chromate of Iron from Bare Hills, near Baltimore.* By the Same.

Its constituent parts are, after roasting,

Silex	10·596
Peroxide of iron	36·004
Alumina	13·002
Protoxide of chrome	39·514
	99·116
Loss	·884
	100·000

(Ibid.)

VIII. *Progress of Mineralogy in America.*

The volume of Professor Silliman's Journal which has just been received, shows that this branch of science is becoming a favourite object of pursuit in America. It announces the discovery of some minerals not before found in that country, and of several new localities of the rarer minerals, such as beryl, chrysoberyl, chlorite, fluor spar, satin spar, epidote, yellow oxide of tungsten, both pulverulent and massive, micaceous iron ore, of great beauty; actynolite, rose quartz, red oxide of titanium, sulphate of strontia, sulphate of lead, &c. as well as of several minerals of importance to the arts, as oxide of manganese, white granular marble, plumbago, and hematites.

IX. *New Test for Arsenic.*

Dr. Cooper, president of Columbia College, finds a solution of chromate of potash to be one of the best tests of arsenic. One drop is turned green by the fourth of a grain of arsenic, by two or three drops of Fowler's mineral solution, or any other arsenite of potash. The arsenious acid takes oxygen from the chromic which is converted into green oxide. To exhibit the effect, take, he says, five watch glasses; put on one, two, or three drops of a (watery) solution of white arsenic; on the second, as much arsenite of potash; on the third, one fourth of a grain of white arsenic in the substance; on the fourth two or three drops of solution of corrosive sublimate either in water or alcohol; in the fifth, two or three drops of a solution of copper. Add to each three or four drops of solution of chromate of potash. In half an hour, a bright, clear grass-green colour will appear in numbers 1, 2, 3, unchangeable by ammonia; number 4 will instantly exhibit an orange precipitate: number 5, a green, which a drop of ammonia will instantly change to blue. Dr. Cooper, however, does not recommend that this test should be exclusively relied on, but merely that it should be used in conjunction with others, of which the most unequivocal is certainly the actual exhibition of arsenic in a metallic form.

(Silliman's Journal.)

X. *Conversion of Cannon Balls into Plumbago.*

In July, 1779, a British squadron from New York invaded the coast of Connecticut; and, in order to favour the movements of a military force which had landed, kept up a cannonade in the town and redoubts of Newhaven. During a violent storm in September, 1821, part of a low bank near that town was undermined by the sea, and a cannon ball discovered which must have lain undisturbed 42 years. The ground in question, where the ball lay, is little else than a salt morass, so that it must have been constantly kept moist by sea water. Its diameter is 3·87 inches. By means of a common saw, a section was easily made through the plumbaginous coat, which, at the place of incision, was half an inch deep, but varied in thickness in different places. The plumbago is cut with the same ease, gives the same streak to paper, and has in every respect the same properties as common black lead.

The same article recounts another instance in which a cannon ball, covered by oysters, adhering firmly to it, was taken from the wreck of a vessel, which appeared to have lain many years under water. When the oysters were knocked off, the external part of the ball was found converted into plumbago, but the central part remained unaltered. It does not, however, appear that this change always happened to cast iron when thus exposed; for an old cannon, found covered with oysters, did not, in the renewal of its coating, shew any signs of such a conversion.*—(Ibid.)

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* In the *Annals of Philosophy*, vol. v. p. 66, (Jan. 1815) may be found a paper by Dr. Henry, on the conversion of cast iron pipes into plumbago. The change seems to have been effected by the action of water containing muriate of soda, and muriates of lime and magnesia.

Article XVI.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

In the press, A Treatise on the Use of Moxa as a Therapeutical Agent, by Baron Larrey; translated from the French, with Notes, and an Introduction containing a History of the Substance, by Robley Dunglison, Fellow of the Royal College of Surgeons.

A Succinct Account of the Lime Rocks of Plymouth, with 10 Lithographic Plates of some of the most remarkable of the Animal Remains found in them, by the Rev. Richard Hennah. 1 vol. roy. 8vo.

A new Edition of Newton's Principia Mathematica, from the best Jesuit's Edition.

A History of a severe Case of Neurelgia, commonly called Tic Douloureux, occupying the Nerves of the Right Thigh, Leg, and Foot, successfully treated, with some Observations. By G. D. Yeats, MD.

JUST PUBLISHED.

The Scottish Cryptogamic Flora, or coloured Figures and Descriptions of Cryptogamic Plants growing in Scotland, and belonging chiefly to the Order Fungi. By Robert Kaye Greville, Esq. FRSE. MWS. &c. Royal 8vo. No. I. 4*s*.

The Philosophy of Zoology, or a General View of the Structure, Functions, and Classification of Animals. By John Flemming, DD. Minister of Flisk, Fifeshire, FRSE. MWS. &c. 2 vols. 8vo. With Plates. 1*l*. 10*s*.

The Naturalist's Repository, or Monthly Miscellany of Exotic Natural History. By E. Donovan, FLS. FWS. &c. Royal 8vo. No. I. 3*s*. 6*d*.

Article XVII.

NEW PATENTS.

P. Erard, Great Marlborough-street, musical instrument maker, for improvements on harps. Communicated to him by a foreigner residing abroad.—April 24.

E. Dodd, St. Martin's-lane, musical instrument maker, for improvements on pedal harps.—April 24.

J. Delvean, Wardour-street, musical instrument maker, for certain improvements on harps.—April 24.

R. Ford, Abingdon-row, Goswell-street-road, chemist, for a chemical liquid or solution of annotto.—April 24.

R. Knight, Foster-lane, Cheapside, ironmonger, and R. Kirk, Osborn-place, Whitechapel, dyer, for a process for the more rapid crystallization, and for the evaporation of fluids, at comparatively low temperatures, by a peculiar mechanical application of air.—May 9.

ARTICLE XVIII.

METEOROLOGICAL TABLE.

1822.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Daniell's hyg. at noon.
		Max.	Min.	Max.	Min.			
th Mon.								
May 1	E	30·30	30·23	66	45	—		
2	N E	30·23	30·05	70	33	—		
3	E	30·06	29·72	70	51	—		
4	E	29·79	29·72	70	41	—	29	
5	N E	29·80	29·79	74	49	54	06	
6	N W	29·83	29·80	73	53	—	03	
7	N E	30·04	29·83	64	44	—	60	
8	N E	30·04	29·84	59	37	—		
9	N E	29·84	29·48	69	41	—		
10	N E	29·65	29·48	59	38	35	14	
11	S E	29·84	29·65	69	37	—	02	
12	N E	29·90	29·84	56	46	—	—	
13	N E	29·95	29·90	58	45	—		
14	N	29·98	29·95	62	47	—		
15	N E	30·05	29·98	76	40	55		
16	N E	30·05	30·02	68	46	—	—	
17	E	30·04	30·02	80	49	—		
18	N	30·11	30·04	77	45	—		
19	N	30·16	30·11	79	45	—		
20	S E	30·28	30·16	81	49	57		
21	N	30·37	30·28	81	47	—		
22	N E	30·37	30·30	76	44	—		
23	N	30·30	30·13	74	44	—		
24	E	30·13	30·04	69	42	—		
25	N	30·04	29·89	72	44	56	33	
26	S W	30·18	29·89	68	44	—	10	
27	S W	30·23	30·18	67	55	—	01	
28	W	30·28	30·23	77	44	43		
29	W	30·35	30·28	76	48	—		
30	W	30·35	30·31	78	48	—		
31	W	30·31	30·23	79	52	44		
		30·37	29·48	81	33	3·44	1·58	

The observations in each line of the table apply to a period of twenty-four hours, ginning at 9 A. M. on the day indicated in the first column. A dash denotes that a result is included in the next following observation.

REMARKS.

Fifth Month.—1. Very fine. 2. *Cirrus:* wind veered to SE, p. m. 3. *Cirrus:* fine. 4. Fine. 5. Very warm: a thunder storm, p.m. 6. Cloudy: close. 7. Rainy. 8. Fine. 9. Cloudy. 10. Showery. 11. Fine. 12, 13. Cloudy. 14—24. Fine. 25. Rain, with thunder, in the afternoon. 26. Showery. 27—31. Fine.

RESULTS.

Winds: N, 6; NE, 11; E, 5; SE, 2; SW, 2; W, 4; NW, 1.

Barometer:	Mean height	
	For the month	30·085 inches.
	For the lunar period, ending the 14th	29·895
	For 16 days, ending the 14th (moon south)	29·929
	For 12 days, ending the 26th (moon north)	30·122
Thermometer:	Mean height	
	For the month	59·516°
	For the lunar period	58·850
	For 30 days, the sun in Taurus	55·612
Evaporation		3·44 in.
Rain		1·58

Laboratory, Stratford, Sixth Month, 24, 1822. **R. HOWARD.**

ANNALS

OF

PHILOSOPHY.

AUGUST, 1822.

ARTICLE I.

Geological Remarks. By Thomas Weaver, Esq. MRIA. MRDS. MWS. MGS.

IN the Comparative View which I took of floetz formations in the British Isles and on the Continent (*Annals of Philosophy* for Oct. Nov. and Dec. 1821), it was my professed object to maintain, that a general order of succession prevails in the structure of the Earth, from the oldest to the newest formations, subject, however, to variation in detail in different countries, and even in the same tract of country, as arising, from a fluctuation of character in particular beds; from the various modes in which correlative formations are associated (namely, as being distinct, or interstratified with each other); and, lastly, from the occasional absence of certain members of a series. In illustration of this doctrine, I produced the carboniferous series as an example, proceeding from the most simple to the most complex arrangements, to be found in the British Isles, and adverting to the local deficiency of particular members of the series. Passing then to the Continent, with the same object before me, I noticed the analogy which subsists between the carboniferous series of England and the Netherlands, and between that of Scotland and some parts of Germany. With this principle, therefore, constantly in view, my surprise was great to find that a writer of distinguished talents had so far misunderstood my observations, as to have conceived that I suppose *an inversion of the order* in the instance of the carboniferous series of Germany, and thus impugned the very doctrine I had undertaken to sustain; while in truth I have not made any such supposition.*

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* See p. 310—319, of "Outlines of the Geology of England and Wales, by the

The question, (involving others of some moment), chiefly depends on the true construction of the term, the *rothe todtliegende formation* of Germany. In the Comparative View, adverted to above, I have considered it as the representative of the carboniferous series, extending from the old red sandstone to the coal formation inclusive; while Mr. Conybeare, on the other hand, maintains that it is the equivalent of what Professor Buckland has denominated the new red conglomerate of England, (the same which I have designated by the name of the calcareous conglomerate), stating that the rothe todtliegende is always found above the coal of Germany, and not below it. In support of our respective opinions, we have both appealed to the same authorities; to Lehman, Werner, Karsten, Von Buch, Freiesleben, &c. Whence does this great discrepancy arise?

If I show that the rothe todtliegende of those authors agrees in relative position, characters, and associations, with the carboniferous series, this will be positive evidence that I have correctly rendered their meaning; and if I further show that the rothe todtliegende is deficient in those particulars that serve to characterise the new conglomerate, this will be negative evidence. The two it is presumed will be deemed conclusive.

Lehman, in his work (Geschichte von Flötzgebirgen, 1756), of which a French translation appeared a few years after, speaks of the rothe todtliegende as "*la base sur laquelle sont appuyés les lits du charbon de terre*" (sect. iv. p. 268, 282); and in describing sections of strata in the following division of the work, he also says, the coal is covered by true rothe todtliegende, meaning that the coal is *imbedded in* rothe todtliegende; and this is the sense in which that author is understood by German writers in general, and by Freiesleben in particular, who expressly quotes him to that effect (vol. iv. p. 170).

Freiesleben, the disciple of Werner, in constant and close intimacy with him to the latest period, and occupying like him a seat in the Council of Mines at Freyberg, may be supposed to represent faithfully the positions of his master, corroborated and elucidated as they have been by his own researches during a residence of seven years in Mansfeld and Thuringia, while acting as chief officer of the mining department in that country. It is well known that the mountainous group of the Hartz consists of primary and transition tracts, whose general constituents I shall now mention, for reasons that will appear hereafter. Granite appears to be the general base, since it occurs not only as such in the primary region, but is found protruding, or denuded, in the transition; in the former also appear clayslate, flinty slate, an intimate compound of felspar and quartz with some little tourmaline called *hornfels*, quartz rock, varieties of trap, limestone, and some indications of gneiss and mica slate. The

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Rev. W. D. Conybeare, FRS. MGS. and W. Phillips, FLS. MGS." an extremely *able and valuable work*, affording, so far as it has proceeded, an excellent view of the *geological relations of the* kingdom. The completion of the task is very desirable.

transition tracts consist of clayslate, greywacke, greywacke slate, and flinty slate, containing subordinately limestone, iron-shot sandstone with impressions of shells, and beds or masses of trap, porphyry, and amygdaloid.

Around this mountainous region are drawn the principal *floetz groups* or *formations* of Werner, not concentrically disposed, as has been sometimes falsely represented, but of unequal distribution; namely, 1. his old or first floetz sandstone, or rothe todtliegende, formation; 2. his first limestone formation; 3. his second or new red sandstone formation; 4. his second or shell limestone formation; 5. his third or quadersandstone formation. Of these, the first four constitute the particular object of Freiesleben's elaborate work, and of three of them, viz. the second, third, and fourth, being the equivalents of the magnesian lime stone (including the calcareous conglomerate), the new red sandstone, and the shell limestone of England, I have given a detailed abstract in the *Annals of Philosophy*. Of the rothe todtliegende formation, I have spoken only in general terms, from the condensed manner in which it was necessary to treat the comparative view of that subject, in the confined space allotted in a periodical journal. Let us now consider it more in detail.

The rothe todtliegende is described by Freiesleben as the old sandstone formation, which contains casually (though rarely in Mansfeld and Thuringia), traces of coal or coaly shale, and incidentally likewise beds of limestone, trap, and porphyry (vol. i. p. 32—34, and 43—46), being also in some parts of its extent in direct connexion and association with the coal formation, properly so called; and hence to this also the term rothe todtliegende is extended (vol. iv. p. 191—198). In a confined sense, therefore, rothe todtliegende signifies the old red sandstone with its subordinate beds, and in a large sense it comprehends the coal formation also, thus representing the whole carboniferous series. It is employed in both these senses by Freiesleben, and other German authors. To obviate, however, all misconception to which this latitude of expression may give rise, I shall, in the following pages, separate the old red sandstone, in the limited sense, from the coal formation, unless where I may employ the term carboniferous series as indicative of the whole. But in a few cases I shall quote Freiesleben's own words to show the latitude in which he uses the term rothe todtliegende.

Relative Position.—The carboniferous series is disposed in the form of a crescent, embracing the foot of the transition tract of the Hartz, on its north-eastern, eastern, and south-eastern sides, ranging thus in a circuit of about 63 miles, from the vicinity of Ballenstadt on the N., past Mansfeld on the E., to beyond Ihlefeld on the S. The only interruption to this continuity is for a short space in the south-western quarter, near Questenberg, where the weissliegende, or new conglomerate, is in immediate contact with the transition country, reposing unconformably on vertical strata of clayslate. The belt, thus described, follows in general in its inclination the declivity of surface presented by the subjacent transition tract, and hence in its line of apposition to that tract, it is sometimes conformable, sometimes unconformable, to the stratification of the latter, the strata in their course dipping successively to the north, east, and south, and at angles varying from 12° to 50°.

The far greater part of this belt consists of the old red sandstone, being connected with coal districts at both its extremities; on the NW. with a coal field about three miles long, extending from the vicinity of Opperode eastward toward Meisdorf, and on the SW. with a field which, commencing in the territory of Stollberg, and ranging past Neustadt to the NW. of Ihlefeld, extends about 10 or 12 miles in length.

The exposed breadth of the old red sandstone is in most parts inconsiderable, being soon concealed by the succeeding floetz formations; but in the south-eastern quarter, where it throws out an arm toward Hornburg on the SE, a distance of about 10 or 12 miles, it acquires in its widest part a breadth of three or four miles, forming a plateau of great thickness and considerable elevation, in the central part of which the strata are nearly horizontal, while on the south-western side the dip is SW. 12° to 15°, at the south-eastern extremity a few degrees to the E. or SE.; and on the north-eastern side, 12° to 30° to the NE.

In the north-eastern quarter, near Hettstädt, the old red sandstone throws out another arm, extending eastward beyond the banks of the Saale in the form of a narrow ridge, about 14 miles in length, in which the prevailing dip of the strata on the southern side is to the S. or SW.; on the northern to the NE.; and in the eastern quarter to the E. At this extremity it is found again connected with and supporting a coal district (vol. iv. p. 191—198), which, as far as exposed, between Kathau and Löbegün on the N. and Dölau and Halle on the S., is about 12 miles long.

In thus following the circuitous course of the old red sandstone, we find it to extend through a range of about 60 miles, to which, if we add the coal fields at its north-western, eastern, and south-western extremities, the entire range of the carboniferous series may be said to be between 80 and 90 miles. But if we consider that on the S. of the general range are to be found several isolated hills of old red sandstone, emerging from beneath the newer floetz formations, e. g. in the Kiffhäuser, &c. we may conclude that the carboniferous series occupies a great expanse also, though mostly withdrawn from observation.

Characters of the Old Red Sandstone (vol. iv. p. 67—119).—I have already noticed the general constituents of the primary *and transition* tracts of the Hartz; and in examining the ingredients of the old red sandstone, we shall find them referable to the detritus of those tracts, and varying in different quarters. The cement which combines these ingredients is usually siliceous, or an indurated sandy micaceous clay; it is more rarely marly, but it contains in general so much oxide of iron as to take a brownish-red colour, whence, as Freiesleben observes, the name of the rock, the *rothe* todtliegende, the *red* dead lier, has been derived, although beds of greyish and whitish colours are not unfrequent.

The sandstone formation itself is described by Freiesleben as consisting of *conglomerate, breccia, sandstone, slaty micaceous sandstone, indurated slaty clay,* and *clay marl,* in beds frequently alternating with each other, from a few inches to several ells, and even fathoms, in thickness; but, generally speaking, they are from two to four feet thick.

The *conglomerate* usually forms thick beds in alternation with finer grained and clayey beds, but sometimes appears in the form of thin layers in fine grained sandstone. It constitutes the least part of the formation, being commonly situated in the lower region. The boulders and pebbles vary from the size of one foot in diameter to that of a nut, compacted by smaller grained ingredients of the same composition, with brownish-red ironshot indurated sandy or marly clay, which is more rarely of a white cast and calcareous quality. The rounded and angular fragments noticed by Freiesleben are clayslate, talcslate, flinty slate, lydian stone, greywacke, gneiss, trap, porphyry, quartz, hornstone, jasper, and agate, with disintegrated felspar, grains of quartz, and mica, differently assembled in different parts of the range; for the minute detail of which, as well as of the following beds, I must refer to the copious work of that author.*

The *breccias* and *sandstones* exceed in mass the conglomerate, and extend to a greater distance from the fundamental rock. A common form of the fine grained *breccia* is that of angular grains of quartz, with single pebbles of quartz and hornstone, and numerous small fragments of yellowish-white decomposed felspar, and scales of mica; sometimes also including single fragments of ironshot clayslate, or talcslate, the whole being combined by a brownish-red sandy cement. Sometimes also small grains of reddish-white decomposed felspar predominate, which, with grains of quartz, are imbedded in a sandy clayey slaty base of a dark cherry-red colour. The small and fine grained *sandstones* often appear of a homogeneous character, and either granular or slaty, brownish-red or grey, more rarely white, sometimes also alternately striped red, grey, and white, or with a reddish-grey or bluish base, containing white spots. In the most uniform sandstone appear occasionally streaks or layers of coarser grained, or of conglomerate, arranged at certain distances in parallel order. The fine-grained sandstones yield large flagstones.

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* In this composition of the conglomerate, we perceive a close analogy to that of some parts of Ireland noticed by the writer of this article, e. g. adjoining Waterford Harbour. (Geol. Trans. vol. v.) Some of the conglomerates also closely resemble those of the old red sandstone of *Tortworth* and Milbury Heath, in Gloucestershire.

The fine-grained clayey sandstone passes into *slaty micaceous sandstone* of different degrees of firmness, containing numerous scales of white mica. It passes also gradually into *indurated clay*, which is mostly slaty, and either pure, or sandy and firm, and of red, green, or grey colours; the substance being generally intimately mixed with minute scales of mica. Beds of this clay, which are often of the nature of *clay marl*, and occasionally contain lighter-coloured portions of a calcareous quality, are frequent in the old red sandstone of Mansfeld.

The *clay* and *clay marl* are found likewise in thin layers between the sandstone beds, of red, grey, green, and blue colours, and sometimes also included in them in the form of ovoidal or flattened elliptical nodules, then resembling, as Freiesleben observes, the clay galls of the new red sandstone formation.

Beside the colouring matter of the old red sandstone derived from oxide of iron, diffused through its substance, brown and red iron ochres are occasionally found in it in spots, round portions, and thin streaks.*

Grains, slight layers, and indeterminate portions of compact, scaly, and ochraceous red iron ore, have also been met with; and near Mansfeld, Freiesleben noticed in the sandstone compact red iron stone in the form of the stems of reeds.

Associations of the Old Red Sandstone.—a. Coal; *b.* limestone; *c.* porphyry and amygdaloid; *d.* coal fields.

a. Coal.—Near Grillenberg, in Sangerhausen, a slight coaly seam, from a half to one and a half inch thick, has been found in the old red sandstone; and a somewhat similar appearance is said to have occurred near Möllendorf, in Mansfeld.†

b. Limestone.—Of the occurrence of this mineral in subordinate beds in the old red sandstone, the following instances are given: near Cressfeld, a bed of compact, splintery, and partly foliated granular limestone, of greyish colours, with interspersed laminæ of white calcareous spar: at Vatterode, Wimmelrode, and Möllendorf, a bed, eight to ten feet thick, of compact fine splintery limestone, of brownish-red, greenish, and reddish-grey colours, with disseminated spots of calcareous spar, and veins of the same substance; it was formerly wrought for marble, and sent to Berlin: near Rothenburg on the Saale, limestone disposed in the sandstone in thin beds, of a red or light-grey colour, minutely granular or compact, and containing terbratulites and discites; while a little further south, Freiesleben was assured, after he had left the country, that the sandstone contains also beds of *oolitic limestone,* where it underlies the Wettin coal field; for the truth of this, however, the author does not actually vouch.

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* The preceding general description of the sandstone, slaty sandstone, clay, and clay marl beds of Mansfeld, with nodules also resembling clay galls, might almost serve word for word, for that of the old red sandstone on the banks of the Severn, in Gloucestershire, as well as in many respects for that of Tortworth and Milbury Heath.

† A parallel instance of the casual occurrence of imperfect coal in the old red sandstone may also be found in England, e. g. in Portishead Point, near Bristol.

c. Porphyry and Amygdaloid.—Porphyry occurs in great lying masses, of an indeterminate form, in the old red sandstone, on the SE. bank of the Wipper between Hettstädt and Burgörner, its immediate cover being coarse sandstone conglomerate. It is traversed by innumerable small veins of calcareous spar.

Not far from hence, porphyry becomes again visible near Meisberg, forming cliffs, protruding 56 to 70 feet high from amidst the old red sandstone. It is partly amygdaloidal, and reposes immediately on coarse siliceous conglomerate.

It re-appears in a similar manner between Hettstädt and Gerbstädt; and porphyry has also been traversed under the same circumstances by the adit level of the mine Johann Friedrich.

The old red sandstone, when adjacent to porphyry, acquires not unfrequently a porphyritic aspect.

d. Coal Fields.

1. *The North Western* (vol. iv. p. 227—237).—On this Freiesleben remarks, "It is quite certain that the coal which occurs near Opperode, in the Principality of Anhalt-Bernburg, lies in the rothe todtliegende." In proceeding from the vicinity of Opperode on the N. toward the transition tract on the S., the beds succeed each other in the following descending order:

1. Brownish-red, ironshot, fine-grained, *rothe todtliegende,* with occasional larger grains of quartz, clay slate, and flinty slate; regularly stratified, strata 1 to 1½ inch thick, dipping 15° to the NW. and affording excellent building stone, 7 fathoms to 9 fathoms 2 feet thick.
2. *Conglomerate,* of unequal grain, iron-shot, some of the pebbles of quartz and slate being 8 and 9 inches in diameter; 32 inches thick.
3. *Sandstone,* same as No. 1, alternating with thin layers of reddish-grey marly rock; 35 fathoms thick.
4. *Slate clay,* bluish-grey, and sandy, with numerous impressions of vegetables; 5 fathoms 5 feet thick.
5. *Roof shale,* bituminous; 1 to 2 feet.
6. *Coal;* 2 feet.
7. *Floor shale,* resembling that of the roof; 3½ feet.
8. *Conglomerate,* very coarse grained; 4 fathoms 4 feet.
9. A second seam of *coal,* but not worth working.
10. *Blue rock,* resembling No. 4.
11. *Conglomerate,* coarse-grained, being the lowest of the series.

The upper coal seam dips pretty rapidly near the surface, but in descending, it maintains a general inclination of 15° to 20°, seldom forming troughs or saddles. The coal field is subject to faults.

2. *The Eastern or Petersberg tract.*

a. The Wettin Collieries (vol. iv. p. 237—260).—These collieries, which are distinguished into the lower or Wettin, and the upper or Schachtberg fields, are generally relieved by an adit level between four and five miles in length, which attains a depth of about 45 fathoms from the surface. The beds of the two fields are very different in different places. The following is said to be the general arrangement in the lower field:

1. Vegetable soil and loam; 2 fathoms 2 feet thick.
2. Sandstone, white, micaceous; 1 fathom 3 feet 6 inches.
3. Clay marly rock, brown; 7 fathoms.
4. Ditto blue; 1 fathom 6 feet thick.
5. Slaty sandstone, bluish-grey, micaceous; 1 fathom 1 foot.
6. *Coal*, upper seam; 6 feet 2 inches.
7. Grey sandy clayey rock; 3 fathoms 3 feet.
8. Coarse sandstone; 3 fathoms 3 feet.
9. Blue slaty rock; 21 to 52 inches.
10. *Coal*, middle seam; 8 inches.
11. Grey sandstone; 3 feet 6 inches.
12. Sandy clay; 1 fathom 1 foot.
13. Blue slate clay with numerous impressions of ferns, reeds, &c.; 5 feet three inches.
14. *Coal*, lower seam; 10 inches.
15. Grey sandstone; 11 fathoms 4 feet.
16. *Rothe todtliegende.*

But considerable variations are to be found in several parts of the fields: thus, instead of the slaty sandstone, No. 5, the immediate roof of the upper coal seam consists not unfrequently of a bed of coarse conglomerate, 2 fathoms 2 feet thick, containing boulders of porphyry of the size of the head, and even larger. The roof of the coal seams is also often of a calcareous nature, consisting of sandstone combined by carbonate of lime, in which pure limestone occasionally appears, and generally in the form of geodes. The middle and lower seams, which are commonly between two and three fathoms asunder, sometimes approach within 10 inches of each other, coalesce, and bear good coal of considerable thickness. The coal seams are in some places greatly enlarged; in others closely compressed, so that they almost disappear. This is the case with the upper seam, which gradually closes and terminates both in the line of range and of dip.

The coal seams of the Wettin district in general are remarkable for the great variableness of position, to which they are subject within short distances, presenting a continued succession of troughs, saddles, and serpentine curvatures, in which the angles of inclination vary from 70° or 80° to the horizontal.

On the Wettin coal field, Freiesleben observes, that "it is imbedded in the rothe todtliegende, being both covered by, and reposing on, rothe todtliegende." The coal has been pursued for 70 fathoms beneath porphyry, and this porphyry is also proved to lie in the rothe todtliegende, not only by a distinct graduation from the one rock into the other, but by the former appearing in separate beds in the latter.

b. *The Löbegün Collieries* (vol. iv. p. 260—264).—These are situated among hills of porphyry, occupying hollows and dells, and apparently forming a mantle round porphyry. Hence no general range or dip can be given; the latter is often at a high angle, from 50° to 80°. It is stated that, wherever accurate observations have been made in these collieries, the porphyry has always been found subjacent to the coal field. The coal beds here, as far as sunk into, consist as under:

1. Vegetable soil, with sand and conglomerate, 21 inches thick.
2. Clay marl varying from 21 to 32 inches.
3. Conglomerate, 7 to 21 feet.
4. *Coal smut,* 3 to 6 inches.
5. Blue clayey rock, becoming gradually thicker in descending, 21 inches to 14 fathoms.
6. *Coal,* 7 feet to 9 feet 8 inches.
7. Blue clayey rock, 21 inches to 14 feet.
8. Blue sandy rock, beyond which the works had not extended.

The coal seam is divided into three beds by two intervening layers of shale, one of which is 10 inches, and the other from 5 to 10 inches in thickness. In another part of the field, a second seam of coal has been met with, from 21 to 32 inches thick.

c. At *Kathau* the coal sandstone is found supporting porphyry on its southern side in a distinct manner (vol. iv. p. 118). At Gerwitz, a small coal trough reposes on porphyry; and between Halle and Giebichenstein the coal is imbedded in porphory. (Vol. iv. p. 293.)

d. Beside these facts, Schulze states in his map, appended to Freiesleben's work, that the coal at Raunitz reposes on porphyry, while to the E. of Brachwitz, it appears to underlie porphyry.

Now, combining these observations, it is perfectly clear that in the coal tract of the Petersberg, between Halle on the S. and Kathau on the N., the coal formation alternates with porphyry; and yet it is stated ("Outlines," p. 469), that these porphyries, thus connected with the rothe todtliegende, belong to the same era as the new red conglomerate or sandstone of England; positions quite irreconcileable with each other.

3. *The South Western Coal Field.*—After the detail into which we have entered, it may be sufficient to observe that in

this district porphyry and trap are also found in association with the coal formation, and that the same construction applies here as in the Petersberg tract.

If now called upon to draw a parallel between this carboniferous series and any of the British, I should say that here is a general tract of old red sandstone, supporting on its eastern confines a coal district, which in many respects agrees with some of those of the Scotch great coal tract; while in the north-western and south-western quarters, the coal fields there appear to repose either immediately on transition rocks, or partly on these, and partly on old red sandstone, corresponding in this respect with some of the coal tracts in Shropshire, as well as in the circumstance of the absence of the carboniferous limestone. I do not perceive any ground that can be laid for considering, as it has been suggested, this range of sandstone as the millstone-grit and shale, (namely, the sandstone and shale interposed in some tracts between the carboniferous limestone and the great coal formation, properly so called), unless it could be shown to be divested of all those general characters of the fundamental portion of the old red sandstone, which it in fact so strikingly displays. On the other hand, the absence of the millstone-grit and shale in Mansfeld and Thuringia is no greater anomaly there than it is elsewhere, e. g. in Cumberland, in most parts of Shropshire, in Ireland, and in the great coal tract of Scotland. Nor is it a greater anomaly that there but few beds of carboniferous limestone occur, since in some parts of our own island they are wanting altogether, e. g. in Shropshire. But this is not the whole of the question. What are we, strictly speaking, to understand by the term old red sandstone? I presume no one in the present day would confine it exclusively to the mere fundamental bed of the carboniferous series. This would be as contracted a view, as if, in the case of gneiss alternating with beds of primary limestone, we were to restrict the use of that word to the lowest bed of gneiss. In the case then of the old sandstone alternating with limestone, where is the line to be drawn? Is it to be extended to the confines of the great coal formation, that is, when the latter is distinct? But who will separate one from the other, when, as in many cases in Scotland, the sandstone, the limestone, and the coal, are repeatedly interstratified with each other? Were we indeed to take a large view of the subject, and to call the British carboniferous series the old or first great sandstone, formation or group, this expression would be quite equivalent to that of the rothe todtliegende, or first floetz sandstone, formation of Germany. In both countries, the subject matter is the same, though, from the fuller display of the seriesin the British Isles, the mode of considering and expressing it has been somewhat different. In making this allusion, however, I am far from meaning to deny that the fourfold division into the fundamental old red sandstone, carboniferous limestone, millstone-grit and shale, and coal formation, is very descriptive, wherever applicable, since it enables us to consider distinctly the several links of a chain, which constitute in themselves a complete system of one great era; but this division cannot always be strictly made, e. g. in most parts of the Scotch great coal tract.

After the full consideration given to the rothe todtliegende formation, extending from the Hartz beyond the banks of the Saale, that of the Forest of Thuringia may be disposed of in few words. The old red sandstone is there found in great force, frequently alternating with porphyry and trap (e. g. vol. iv. p. 107—116), reposing principally on primary rocks, and clearly showing by its composition, like that of Mansfeld &c., that it originated from their detritus (vol. iv. p. 67—99.) The coaly shale and coal formation, connected with the old red sandstone, is unequally distributed, being also occasionally associated with limestone and porphyry, and coming in the course of its extent not unfrequently in contact with primary tracts, the coal field reposing in some instances upon granite (e. g. vol. iv. p. 167, 168).

I will now consider the red sandstone formation described by Von Raumer. But previously let me observe that, as the principal object of that author's researches was to ascertain the grand features and relations of the country, we are not to expect that great precision, or minute detail, that are so generally observable in the works of Freiesleben. Still, however, the leading facts can hardly admit of dispute. For the general positions, I refer to the *Annals of Philosophy* for Oct. 1821, p. 248—250.

The red sandstone formation which occupies so large an expanse on the southern side of the Riesengebirge, extending W. from Schatzlar, appears to be destitute of coal. It dips S. But proceeding eastward from that town, it is found connected with an extensive coal field, sandstone conglomerate forming throughout the great basis, the character of which varies according to that of the adjacent primary or transition tracts upon which it reposes, enveloping pebbles and fragments of gneiss, micaslate, granite, hornblende slate, clayslate, quartz, &c. (Von Buch, Geog. Beob. vol. i. p. 85—93; Von Raumer, p. 92.) The carboniferous series ranges along the south-western side of the Eulengebirge toward Glatz, a distance of about 35 miles. Having given a summary of its general relations in the work referred to above, it may be sufficient to add in this place a few remarks on the general disposition of the tract.

This carboniferous *series* is almost entirely overlaid on its south-western side by the quadersandstone formation; in all other quarters, its line of contact with older rocks may be followed, from which it appears throughout to conform to their sinuosities, with a dip corresponding. Thus on the NW. it is generally inclined toward the SE. or S.; on the NE. to the SW. but on the SE. occupying in that quarter three inlets, or bays as it were, the dip is successively directed to every point of the compass except the E. Again, on the SW. near Schlesisch Albendorf, on the confines of the quadersandstone, the dip is to the ENE.; while beyond the quadersandstone on the SW. where the coal re-appears at Straussensee, the dip is NNE., a disposition probably arising from the adjacent primary tract. The general arrangement of the beds, therefore, indicates the form of a great trough, at least in the south-eastern quarter; but the internal structure of the field is rendered very intricate and complex from the interposition of isolated ranges and masses of porphyry and trap, which have a sensible influence on the stratification of the country. The map of the environs of Waldenburg, founded on actual survey, is an evidence of this fact. Similar indications appear in the south-eastern part of the tract, and hence the course of the coal and concomitant beds becomes frequently curve linear, dipping at angles varying from 80° to 15°. That the seams of coal are extremely numerous, and that there is an interstratification of the whole series of beds connected with the coal, may be fully inferred by combining the observations of Von Raumer with those of Von Buch. The latter states that if we traverse the outcrops of the strata from Fürstenstein to Albendorf, we shall fall short rather than exceed the number, if we reckon the beds of the carboniferous series at 500; that is, without including the innumerable beds that extend but a short way, edge out, and are lost among the others (Geog. Beob. vol. i. p. 101—103). The number of beds of limestone in the series appears not to be ascertained, for though limestone has been noticed in 22 places, several of these spots seem to be situated on the line of bearing of the same stratum. It would require laborious and expensive research before a correct map, with corresponding sections, of this coal tract could be constructed; and the sections of Von Raumer can only be considered as illustrative diagrams, tending to convey a general idea of the relative position of the carboniferous series itself, without pretending to give a detailed view of its internal conformation. The red sandstone and porphyry adverted to, as if covering this coal district ("Outlines," p. 470), form in fact a part of the general series.

The structure of this tract seems very analogous to that of the Scotch great coal field. Many of its features correspond also with those of the coal tract of the Petersberg, on the banks of the Saale.

The red sandstone formation situated to the north of the Rie sengebirge ranges, as far as noticed by Von Raumer, for a distance of about 40 miles, from Walkersdorf, between Lauban and Bunzlau on the NW., to beyond Bolkenhayn on the SE., where it terminates. It appears disposed in the form of a trough in a primary slaty tract, by which it is encompassed on every side, except on the greater part of the northern, where it is overlaid by the quadersandstone formation. The dip appears to conform to the indentations of this trough, at angles varying between 30° and 60°. The predominant rock is the red sandstone, but beds of compact limestone, of reddish and yellowish-grey colours, are not unfrequent in it. The sandstone alternates also with felspar porphyry, claystone porphyry, basaltic trap, and amygdaloid. Only one slight trace of coal has been observed in it, and Von Raumer, in his general view of this tract, no where remarked any organic remains. It is referred by Von Buch to the old red sandstone formation (Geog. Beob. vol. i. p. 77, 78).

The preceding statements have, I trust, proved; 1. The identity of the old red sandstone of Werner, and that of the British Isles; 2. That the rothe todtliegende formation of that naturalist is the representative of the carboniferous series; and 3. That in the details of that series in Germany, we perceive the occasional absence of a particular member, and various states of association, in the same manner as they are to be found in Britain, but *no inversion* of the general order.

These positions being thus established, in what sense can the rothe todtliegende be said to be the same as the new conglomerate? I do not know any, except by a misapplication of terms. The weissliegende of Germany, which in all its relations of position, composition, and association, perfectly corresponds with the new conglomerate, has been repeatedly called rothe todtliegende by Voigt and other writers, down to the latest period, who, considering it as the uppermost bed of that series, have, without due attention to their different characteristics, confounded the two together. This fact is insisted on by Freiesleben, to whom we owe the clear exposition of this error (vol. iii. p. 239)*; an error that might readily be committed in the Forest of Thuringia in particular, where the new conglomerate (the weissliegende of Freiesleben) is mostly of a siliceous character and reddish hue, nearly resembling in aspect the rothe todtliegende; and as the latter is there in frequent association with trap and porphyry, and commonly supports the new conglomerate, the

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[*] A somewhat similar error prevailed in England at no distant period, for proof of which it may be sufficient to refer to Townsend's work, "The Character of Moses vindicated." 1813. See vol. i. p. 154, et seq.

two formations might easily be mistaken for each other, when not duly observing their respective boundaries; and the inference then be drawn that the new conglomerate is also associated with porphyry and trap. Now in adverting to the Forest of Thuringia, it is stated, ("Outlines," p. 316,) "here we may observe, at top a shell limestone, answering to our lias; then red marle and gypsum; the calcareous beds associated with the cupriferous marl slate; and *at bottom* the rothe todte." (See also p. 313.) But this so-called rothe todte at the bottom of the cupriferous marl slate, is assuredly the weissliegende of Freiesleben; that is, the new conglomerate, beneath which is to be found the true rothe todtliegende in association with trap and porphyry.

Here then we have a formation which, as the first member of a new series, covering the carboniferous series, and extending beyond it to the transition series, and even to the primary, distinguished by gypsum as its occasional companion, beside other characters (which I have detailed in the *Annals of Philosophy*, Nov. 1821, p. 255—257), leave no room to doubt its identity with the new red conglomerate of England.

Again, with respect to the supposed alliance of the new conglomerate with porphyry and trap, I confess I do not know an instance of the kind in Germany; and Freiesleben is perfectly silent upon any such occurrence. Of its existence in England, only one example is given, and that is admitted to be of a problematical character. I have suggested that the amygdaloidal trap in the neighbourhood of Exeter might possibly be connected with the transition tract of that country: this seems to be denied. Yet, as presenting some ground for the suggestion, I must remark that the very able and luminous view of Cornwall, Devon, and Somerset, taken by Prof. Sedgwick, in the first volume of the Cambridge Phil. Trans., has clearly shown that red transition conglomerate and sandstone occur at least in Somersetshire; to which I may add that transition red sandstone is of common occurrence in Gloucestershire and Herefordshire, being in the former county in direct association with amygdaloidal trap, which has sometimes also a porphyritic aspect, including acicular crystals of glassy felspar. If, however, the conglomerate and sandstone in question be not transition, I then venture to inquire whether it may not be the first floetz or old red sandstone? Of the existence of the latter in Somerset, I was assured three years since by the researches of my friend, the Rev. Dr. Cooke, who found it supporting the carboniferous limestone of Cannington Park; a view now apparently confirmed by the high authority of Mr. Conybeare. In support of either suggestion, it may also be stated that no writer appears to have observed any gypsum in the sandstone said to be associated with the trap, while it is not uncommon in the extensive tracts of the new red sandstone of that part of the kingdom, e. g. in Devon, at Budleigh Salterton near Teignmouth, at Sidmouth, and on Blackdown, and again more N. in Somerset adjacent to the Quantock Hills. I may here incidentally observe that both in Devon and the SW. of Somerset, the magnesian limestone formation, properly speaking, appears to be wanting in the regular order of succession, between the new or calcareous conglomerate and the new red sandstone.

Having thus entered my decided protest against considering the new red conglomerate of England, and the rothe todtliegende of Werner, as equivalent terms, I now proceed to perform a similar task in respect of the old red sandstone and carboniferous limestone of England; the former of which, it is said, is a variety of the greywacke of Werner, and the latter his transition limestone; and upon this view, the charge is raised that the *Wernerians* have confounded the carboniferous series with the transition. Is this charge just?

In the Netherlands the two series certainly have been confounded together; but by whom primarily? by French writers on that tract, e. g. Omalius d'Halloy and M. Clere, neither of whom, I presume, will pronounce himself to be of the Freyberg school. It is true, Von Raumer, in his Geognostic Sketches in 1815, has quoted Omalius d'Halloy's statements without inquiry, and D'Aubuisson has done the same in his Traité de Géognosie in 1819, and to that extent they, as well as other Continental writers citing to the same effect, are doubtless chargeable with the mistake. Yet are such oversights, springing from a foreign source, to be visited on Werner and his followers? Has Werner himself, or Von Buch, or Freiesleben, for instance, confounded the carboniferous series with the transition? I do not anticipate an affirmative to this question. In fact, how can the old red sandstone of Britain, which I have shown to correspond in all its relations, perfectly with the old red sandstone of Werner, be held to be a variety of the greywacke of that naturalist (even putting mineralogical character out of the question), or how can the carboniferous limestone be said to be his transition limestone, when both in his view occupy totally different positions?

It is very true, and must be admitted by all conversant with the subject, that the red sandstone and the limestone of the carboniferous series often closely resemble the red sandstone and the limestone of the transition series, so much so, as in hand specimens to be scarcely distinguishable from each other, and this similarity is further increased by several kinds of organic remains being common to both limestones; and it is also true, that what have been called graduations from one series into the other may be observed in certain situations, and so far appearances may be deceptive. Yet no attentive geologist can be deceived in this particular, if he take that view of the subject which ought always to be taken; namely, if he follow throughout the line of contact between the carboniferous series and the transition. He will then readily perceive that the two series constitute totally distinct systems; for if the former appear in some places conformable in position to the latter, these are merely local occurrences arising from the more variable stratification of a transition country, while in the general arrangement unconformability of position will be found to prevail. And as a subsidiary mark of distinction, it may also be added, that the transition sandstone frequently contains organic remains, while the first floetz sandstone is generally free from them.

Von Hoff, however, did propose to incorporate the carboniferous series with the transition (in Leonhard's Taschenbuch, Jahrgang, viii. p. 320—328); and if some other writers have partly leaned the same way (whether justly called Wernerians, I will not stop to inquire), this disposition cannot be charged as derivable from Werner, whose positions are irreconcileable with any such attempt.

It follows from the whole of these premises, that the floetz formations of Werner strictly commence with the old red sandstone of England, and not, as has been stated, with the new or calcareous conglomerate. It follows also, that the charge of confusion in the views of that naturalist is obviated, and that so far from the floetz formations which came under his consideration having been few in number, they comprehended the whole series from the old red sandstone up to the chalk, and above the chalk, gravel, sand, clay, wood-coal, and the newest floetz trap formation. His arrangement of formations in Germany is, when duly construed, quite in accordance with their succession in the British Isles; there is no hiatus; we travel from the primary to the transition, and thence through the whole series of the floetz, in which last let it be observed, that though the carboniferous series be less fully displayed, yet other formations are in much greater force in Germany, and afford a greater variety of character than is to be found in the British Isles; and here we may perceive the compensating power of nature.

I have, therefore, yet to learn that more modern inquiries have at all invalidated the general positions of Werner. His grand outlines of the structure of the globe remain unshaken, from the fundamental granite up to the newest floetz trap. The labours of his followers, and of other geologists pursuing a similar path, have tended more and more to fill up those outlines. In our own country, few, if any, are entitled to greater distinction in that respect than Mr. Smith, whose views also have so far the merit of originality as they appear not to have been derived from any extraneous source. The later investigations of numerous English naturalists, of M. Greenough, Dr. MacCulloch, Professors Buckland and Sedgwick, Messrs. Webster, Conybeare, Miller, Phillips, De la Beche, beside those of a host of British writers whose names are recorded either in their own distinct works, or in the Geological, Philosophical, and Wernerian Transactions, or in periodical publications, are all invaluable contributions to the same effect. Above the chalk, the history of our planet has been further elucidated by the inestimable researches of Cuvier, Brongniart, Mr. Webster, Prof. Buckland, and other geologists pursuing that branch of the subject. But all these labours, whose merits and importance can never be too highly appreciated, so far from impugning the general facts advanced by Werner, serve rather to confirm and establish them.

The Comparative View of floetz formations, which I submitted to the public in the *Annals of Philosophy*, Oct. 1821, is consistent with the main positions of Werner, though, from the mode of considering them, there may seem to be some difference: this, however, is rather apparent than real. It arises from the following circumstances: 1. In the carboniferous series, producing the limestone and the coal as distinct formations, while Werner considered them only as members of his first floetz sandstone, or rothe todtliegende, formation: 2. In like manner, in the gypseous and saliferous series, producing the weissliegende or calcareous conglomerate as a distinct formation, while by Freiesleben and others it is included in the magnesian limestone formation: 3. As a consequence of the foregoing, in considering the magnesian limestone as belonging to the second floetz series: and 4. From distributing the floetz formations into four principal series, founded, as I conceive, on natural distinctions; namely, on their relative position in the order of succession, their mineralogical characters, the organic remains which they respectively contain, and the mutual affinities of the formations which constitute each series or group. In this view there is no real incongruity; for, in fact, had the carboniferous limestone appeared in force in the north of Germany, it certainly would have been designated by Werner as the first floetz limestone; and this, according to the established method of that naturalist, who, in arranging the mineral masses of the globe, was led to distribute the predominant into *principal* formations, and the incidental into *subordinate*. Bearing this in mind, the carboniferous limestone would have been his first floetz limestone formation, and as a necessary consequence, the magnesian limestone would have become his second floetz limestone. The whole difference, therefore, is a mere question of enumeration.

Here let me add a few words on the meaning of the term floetz. It was employed by Lehman, and adopted by Werner. English writers have repeatedly asserted that it signifies flat or horizontal. Such is not necessarily its import. The French translator of Lehman more nearly expressed its sense by *roches en couches*. Floetz literally signifies a *mineral bed*, and floetzgebirge bedded or interstratified formations, as more peculiarly characteristic of those mineral masses to which the term has been applied. The general tendency to horizontality, increasing from the older to the newer floetz formations, is, it is true, a distinctive mark of these formations, but still horizontality is not necessarily implied in the word floetz. And, even if it were, the occasional departure from the horizontal position would be no more an objection to the use of the term, than the occasional horizontal disposition of primary strata would be to their general designation as inclined. All that can be said is, that in both cases the general rule is subject to exceptions.

To the continued use of the term floetz, as applied to any part of the carboniferous series, an objection has been raised upon the supposition that the original sense in which it was employed has been departed from (*Introduction*, p. vi. and *Outlines*, p. 352); but as that supposition has been shown in the course of this paper to rest wholly on a misconception of the true import and application of the term, the objection vanishes. I may further add, that the value of a word consists in its conveying a definite idea to the mind, and so long as terms of established usage thus perform their office (in which respect the word floetz is not deficient), to exchange them for new can only be justified by showing that the latter answer the purpose better.

In conclusion I must observe, that in awarding the meed of praise due to the services of Werner, French writers appear in general to have been more just than the English. Not a few of the latter seem to forget, or not to consider, that though others might before his time have hit upon the general division of rocks into primary and secondary, yet geology, as a science, had no existence. To Werner belongs, in the first place, the merit of introducing a nicer discrimination in the examination of simple minerals, and of inventing an appropriate language by which they might be described and distinguished, previous to which mineralogical science was quite in its infancy. And, in the second place, to him also belongs the chief merit, not merely of distinguishing and giving names to rocks, but of accurately marking out both the grand distinctions of primary, transition, and floetz classes, and the various principal formations of which those classes consist. If then it be the glory of the Saxon to have laid the broad foundations of the edifice, let that of the Briton and Frank be to complete the structure.

ARTICLE II.

Extracts from the "Journal of a Survey to explore the Sources of the Rivers Ganges and Jumna." By Capt. J. A. Hodgson, 10th Reg. Native Infantry.

(Concluded from p. 52.)

A sharp peak across the river; call it the pyramid. Height above the —— 20,966 feet.

A rock on the great snowy bed, over which we are to pass, proved to be distant 9044 feet, and its height above this place 984 feet, the angle of elevation being 6° 15′, which is the general inclination of the snow bed; as our progress was continued far beyond this rock, it will easily be imagined that the crest or summit of the bed, *then distant* five or more miles by estimation, must have considerable elevation.

We had brought very few followers onwards from Gangotrī, but here we sent back every one we could possibly dispense with, that our small stock of grain might subsist the remainder, who were a few trusty fellows (Mussulmans), two Gorc'ha Sipāhīs, and a few Coolies, for two days, or three, if possible, in the event of our being able to get over the snow in front. And I sent orders to the people at Gangotrī to leave grain there if they had any to spare, and if they did not hear of any supply coming from Reital, to make the best of their way back till they met it, and then to halt for us, and send some on to us.

Having made all the arrangements we could on the important head of supplies, and made observations, we had leisure to admire the very singular scenery around us, of which it is impossible to give an adequate description.

The dazzling brilliancy of the snow was rendered more striking by its contrast with the dark blue colour of the sky, which is caused by the thinness of the air; and at night, the stars shone with a lustre which they have not in a denser atmosphere. It was curious too to see them, when rising, appear like one sudden flash, as they emerged from behind the bright snowy summits close to us, and their disappearance, when setting behind the peaks, was as sudden as we generally observed it to be in their occultations by the moon.

We were surrounded by gigantic peaks entirely cased in snow, and almost beyond the regions of animal and vegetable life, and an awful silence prevailed, except when broken by the thundering peals of falling avalanches. Nothing met our eyes resembling the scenery in the haunts of men; by moonlight, all appeared cold, wild, and stupendous, and a Pagan might aptly imagine the place a fit abode for demons. We did not see even bears, or musk deer, or eagles, or any living creature, except some small birds.

To form an idea of the imposing appearance of a snowy peak, as seen here under an angle of elevation of nearly 33°, and when its distance is not quite three miles, and yet its height is 8052 feet above the station, one should reflect that if even when viewed from the plains of Hindustan, at angles of elevation of one, and one and a half degree, these peaks towering over many intermediate ranges of mountains, inspire the mind with ideas of the grandeur, even at so great a distance : how much more must they do so when their whole bulk, cased in snow from the base to the summit, at once fills the eye. It falls to the lot of few to contemplate so magnificent an object as a snow clad peak rising to the height of upwards of a mile and a half, at the short horizontal distance of only 2¾ miles.

May 31.—Along, and above the right bank of the river, rocks and snow.

Descent to the bed of the river, enclosed by rocks.

A most wonderful scene. The B'hāgirat'hī or Ganges issues from under a very low arch at the foot of the grand snow bed. The river is here bounded to the right and left by high snow and rocks; but in front over the debouche, the mass of snow is perfectly perpendicular, and from the bed of the stream to the summit, we estimate the thickness at little less than 300 feet of solid frozen snow, probably the accumulation of ages; it is in layers of some feet thick, each seemingly the remains of a fall of a separate year. From the brow of this curious wall of snow, and immediately above the outlet of the stream, large and hoary icicles depend; they are formed by the freezing of the melted snow water of the top of the bed, for in the middle of the day, the sun is powerful, and the water produced by its action falls over this place in cascade, but is frozen at night. The Gangotrī Brahmin who came with us, and who is only an illiterate mountaineer, observed, that he thought these icicles must be Mahādēva's hair, from whence, as he understood it is written in the Shāstra, the Ganges flows. I mention this, thinking it a good idea, but the man had never heard of such a place as actually existing, nor had he, or any other person to his knowledge, ever been here. In modern times they may not, but Hindus of research may formerly have been here, and if so, I cannot think of any place to which they might more aptly give the name of a Cow's Mouth than to this extraordinary Debouche. The height of the arch of snow is only sufficient to let the stream flow under it. Blocks of snow were falling about us, so there was little time to do more here than to measure the size of the stream. Measured by a chain, the mean breadth was 27 feet. The greatest depth at that place being knee deep, or 18 inches, but more generally a foot deep, and rather less just at the edges, say 9 or 10 inches; however, call the mean depth 15 inches. Believing this to be (as I have every reason to suppose it is) the first appearance of the famous and true Ganges in day-light, saluted her with a bugle march, and proceeded (having to turn a little back to gain an oblique path) to the top of the snow bed; having ascended it to the left.

Pretty strong ascent up to the inclined bed of snow. This vast collection of snow is about 1¼ mile in width, filling up the whole space between the feet of the peaks to the right and left: we can see its surface forward to the extent of four or five miles, or more, to where it is bounded on the left by the feet of the Four Saints, and to the right by snow spurs from other mountains beyond Mount Moira. These last spurs rather overtop the feet of the Saints, and to them, and to the place where we judge there is a ridge, is all ascent over snow.

Ascent of the same kind; generally, acclivity 7°, but we pass over small hollows in the snow, caused by its irregular subsiding. A very dangerous place; the snow stuck full of rubbish, and rocks imbedded in it. Many rents in the snow appear to have been recently made, their sides shrinking and falling in. A man sunk into the snow, and was got out not without some delay. The bed of the Ganges is to the right, but quite concealed by the snow.

In high hope of getting on to what may be at the top of the acclivity, we have come on cheerly over the hollow and treacherous compound of snow and rubbish, but now with bitter regret, we both agree that to go on is impossible. The sun is melting the snow on all sides, and its surface will not bear us any longer. I have sunk up to my neck as well as others. The surface is more and more ragged, and broken into chasms, rifts, and ravines, of snow with steep sides. Ponds of water form in the bottoms of these, and the large and deep pools at the bottoms of the snow hollows, and which were in the earlier part of the day frozen, are now liquid. It is evident from the falling in of the sides of the rents in the snow, that there are hollows below, and that we stand on a treacherous foundation. It is one o'clock, and the scene full of anxiety and awe. The avalanches fall from Mount Moira with the noise of thunder, and we fear our unsteady support may be shaken by the shocks, and that we may sink with it.

And here we were obliged to return! Had it been possible to have got across the chasms in the snow, we would have made every exertion, so anxious were we to get forward; but onward, their sides were so steep, and they appeared of such great depth, that I do not think it would be possible to pass them (this year at least), even if the snow was not as at this hour soft, and the bottoms of the chasms filling with water. Be that as it may, they are now utterly impassable. At this season snow must fall here whenever it rains below, so that it does not acquire such hardness at the top as it does on the avalanches we have hitherto passed, where no new snow at present falls. We now set out on our return, and not too soon, as we found; for the snow was so soft, and the increase of the water so great, that though we went with the utmost expedition, it was only by 2½ hours' hard labour of wading and floundering in the snow, and scrambling among rocks, where they would give a footing, that we reached the turf, tired and bruised with falls, and the skin taken off from our faces and hands by the sun and drying wind of these elevated regions.

It now remains to give some account of this bed or valley of snow, which gives rise to the Ganges. It appears that we passed up it, somewhat more than a mile and a half. From our last station, we could see onwards as we estimated about five miles to where there seemed to be a crest or ridge of considerable elevation, though low when compared with the great peak which flanked it. The general slope of the surface of the snow valley was 7°, which was the angle of elevation of the crest, while that of the peak of St. George, one of those which flanked it to the left, was 17° 49′. In the space we had passed over the snow bed, the Ganges was not to be seen; it was concealed probably many hundred feet below the surface. We had a fair view onward, and there was no sign of the river; and I am firmly convinced that its *first appearance in day* is at the debouche I have described. Perhaps indeed some of those various chasms and rents in the snow bed which intersect it in all sort of irregular directions, may occasionally let in the light on some part of the bed of the stream, but the general line and direction of it could only be guessed at, as it is altogether here far below the broken snowy surface. The breadth of the snow valley or bed is about a mile and a half, and its length may be six and a half miles, or seven miles from the debouche of the river to the summit of the slope, which terminated our view: as to the depth of the snow, it is impossible to form a correct judgment, but it must be very great. It may easily be imagined that a large supply of water is furnished at this season by the melting of this vast mass in the valley, as well as by the melting of that of the great peaks which bound it. From their bases torrents rush, which, cutting their way under snow, tend to the centre of the valley, and form the young Ganges, which is further augmented by the waters which filter through the rents of the snow bed itself. In this manner, all the Himālaya rivers, whose heads I have visited and passed over, are formed; they all issue in a full stream from under thick beds of snow, and differ from the Ganges in as much as their streams are less, and so are their parent snows. On our return down the snow valley, we passed nearer to its north side than in going up, and saw a very considerable torrent cutting under it from the peaks; this was making its way to the centre: at times we saw it through rents in the snow, and at others only heard its noise. As there must be several more such feeders, they will be fully sufficient to form such a stream, as we observed the

Ganges to be at the debouche in the space of six or seven miles. I am fully satisfied that if we could have gone further that we should not have again seen the river, and that its appearance at Mahādēva's hair, or whatever we may choose to call it, was the real and first debouche of the B'hāgiratt'hī. All I regret is that we could not go to the ridge to see what was beyond it. I suspect there must be a descent, but over long and impassable wastes of snow, and not in such a direction as would lead direct to any plains, as the course to bring one to such plains would be to the north-east or north, whereas the line of the river's course, or rather of the ridge in front, was to the south-east, parallel to the run of the Himālaya, which is generally from SE to NW. Immediately in front of the ridge, no peaks were seen, but on its south-east flank, and at the distance of about 18 miles, a large snowy peak appeared, so that I think there can be no plain within a considerable distance of the south-east side of the ridge: if there be streams from its other side, they must flow to the south-east. After all, I do not know how we should have existed, if we had been able to go to the ridge, for we could not have arrived there before night; and to pass the night on these extensive snows, without firewood or shelter, would have cost some of us our lives, but of that we did not then consider much (if we could have gone, we would). We had only a few trusty men with us, and a short allowance of grain for them, for this and the following day, and had sent orders to the people left at Gangotrī to make their way back towards Reital, leaving us what grain could be spared, and to forward what they might meet, as I expected some from Reital, from whence we were supplied during our absence from it of altogether 28 days. I cannot suppose that by this way, there can be any practicable or useful pass to the Tartarian districts, or doubtless the people would have found it out, and used it, as they do that up the course of the Jāhnavī. While I give it as my opinion, that under any circumstances the crossing of the ridge must be difficult, I would by no means wish to be understood to assert that I think it impossible under more favourable circumstances, and in a year when less snow has fallen than in the present; but I seriously declare, that situated as we were, it was not possible for us to go further than we did, and that it was with great difficulty we got back.

It is now to be considered, if the supplies of water produced as above described, are sufficient to form a stream of 27 feet wide, and 15 inches (mean depth) at the debouche. It has been stated that at Gangotrī, the breadth of the river on the 20th of May was 43 feet, and its depth 18 inches. The distance thence to the debouche was 22,620 paces, which I reckon about 11 British miles. In that space, it received some supplies, as mentioned in the notes, but they were not abundant. Thus the quantity of water is diminished nearly one half; but it is to be remembered that on our return to Gangotrī on the 2d of June, the bulk of the river was considered as being doubled, it being two feet deep, and also much wider, so that on the 31st May, we may suppose it to have been 21 inches deep, and perhaps 48 feet wide at Gangotrī. It is with this mean size that the comparison of the difference of its bulk at Gangotrī and the debouche must be made; the proportion thus is, that the body or quantity of water would be at Gangotrī almost treble to that at the debouche; but allowing it to be only double in this 11 miles, it will be evident that in five or six miles further, there can be little or no water in the bed under the snow, and consequently that the most remote rill which contributes under the snow to the first formation of the Ganges cannot be more distant than the ridge; so I think it may be allowed that such first formation is on the hither side of the ridge, and not at any lake, or more distant place beyond it.

Indeed considering the large supplies which the snow valley furnishes, I rather wonder that the stream was not larger, when I measured it at the debouche. Whether there are any boiling springs under the snow as at Jumnotrī I do not know, but suppose there are not, as I did not see any smoke; a steam, however, there may be, and the steam may be condensed ere it can appear. I imagine that the season of the rains would be in one respect the most proper to attempt the passage of the great snow bed; it may at that time be reduced in thickness; but I have no idea that it ever melts away; yet in the rains it perhaps will not be possible to ford the river above Gangotrī, which must frequently be done, if the smaller avalanches on which we very frequently crossed it are melted. In the rains also there must be greater hazard from the falling of the rocks and slips of the mountain, for the melting snow forms many rills which undermine the rocks, and set them loose, and it is not possible to avoid a large fall of the mountain's side, if one should unfortunately be in the line of its direction when it comes down.

I have preserved specimens of the rocks of which these peaks are composed; also of the different sorts of pines which grow at their basis. Above Suc'hī and Jhala, the country is not inhabited, nor is it habitable beyond those places, except at the small village of Durāli, which is now deserted. Tuwarra, Suc'hī, and Jhala, are very small and ruinous villages. Reital is a pretty good village of about 25 houses, as is Salung, and there are two or three more in that neighbourhood. I found the inhabitants civil and obedient.

The people of Rowaen are in general much inferior in appearance to those of Jubul and Sirmour, and the more western mountains; indeed, with few exceptions, they are an ugly race both men and women, and extremely dirty in their persons. They complain much of the incursions of the banditti from the western parts of Rowaen and Busahir, who carry off their sheep in the rains; but from what I can learn, they in turn plunder their eastern neighbours of the Cēdar-nāt'h districts, and they pride themselves on the long journeys they make in their sheep stealing expeditions. The proper time for those incursions is the latter end of the rains, when the snow in the defiles is much reduced. The women have not here, as to the westward, a plurality of husbands. I saw no fire arms among the inhabitants, nor swords or war hatchets; their weapons are bows and arrows. The climate of Reital is at this season very pleasant, and the price of grain is not high, but it is not abundant. The corn is cut in the beginning of June.

No volcanos were seen or heard of in these mountains, whose composition is granite of various kinds and colours. No shells or animal remains were seen. The magnetic variation was small, and differing little, if at all, from what it is on the plains of the upper provinces; it is from 40′ to 1° and 2° according to different needles, and is easterly, by which I mean that the variation must be added to the magnetic azimuth. The diurnal small changes in the barometer were perceptible, the mercury always falling a little before noon as in the plains.

Having received new thermometers from Calcutta, both long and short, I found that they gave the same boiling point, but the thermometer I had last year in Busahir, &c. showed the boiling point 2° or $2\frac{1}{4}$° below the new ones. I always suspected the thermometer, but had not then a better. It boiled in the Panwei pass in the Kunaur and Busahir snowy mountains at 188° at my camp a little above the lower line of snow on the 24th June last, so that it should have been 190°, or 22° lower than at the sea side. Bears abound in the higher mountains; also the goorul or boorul, an animal between the deer and goat, and the pheir, a larger animal of the same kind. I have preserved the skin, horns, and bones, of the head of one shot near Jumnotri. Near the villages where snow lies a great part of the year, there are abundance of the Monaul pheasants and chakors. In the lower mountains there are black partridges, and tigers, leopards, and bears. I never saw any snakes in the cooler regions.

It was remarked above, that the snow on the great bed was stuck as it were with rock and rubbish in such a manner as that the stones and large pieces of rock are supported in the snow, and sink as it sinks; as they are at such a distance from the peaks as to preclude the idea that they could have rolled down to their present places, except their sharp points had been covered, it appears most likely that the very weighty falls of snow which there must be here in the winter bring down with them pieces of rock in the same manner as a larger snow ball would collect gravel, and carry it on with it in its course. Masses of snow falling from the high peaks which bound the snow bed, if they chanced to collect more, and to take a rounded form, would have a prodigious impulse, and might roll to the centre of the snow valley, loaded with the pieces of rock they had involved.

It is not very easy to account for the deep rents which intersect this snow bed, without supposing it to be full of hollow places. It struck us that the late earthquakes might have occasioned some of the rents. I never saw them before on other snow beds, except at Jumnotri, where they are occasioned by the steam of the extensive range of boiling springs there; perhaps there may be such springs here also: they are frequent in the Himālaya, and one might suppose they were a provision of nature to insure a supply of water to the heads of the great rivers in the winter, when the sun can have little power of melting the snow above those deep recesses.

I will now proceed to give some account of the course of the river Jumna within the mountains, and of its spring at Jumnotri, which I also visited this year. The above remarks respecting the Ganges having already swelled this paper to too great a bulk, I will make those regarding the Jumna in as few words as possible. In the maps published 10 years ago, the Jumna is laid down as having a very long course from the latitude of 34½°; from what authority it is difficult to guess, for much as has been surmised and written respecting the head of the Ganges, I cannot find any accounts of that of the Jumna. It was not known until the year 1814, that the Jumna, properly so called, was a comparatively small river above its junction with the Tonse in the Dūn, and I believe the existence of the latter river, though fully treble the size of the Jumna, was unknown to Europeans.

The junction of the Tonse and Jumna takes place at the NW. end of the Dūn valley, in latitude 30° 30′, where the large river loses its name in that of the small one, and the united stream is called the Jumna. The course of the Jumna from Jumnotri, which is in latitude 30° 59′, being generally S. 50° W. It is fordable above the confluence, but the Tonse is not. Not having visited the sources of the Tonse, I am not certain whether it rises within the Himālaya, as the B'hāgiratt'hī does, or at its SW. or exterior base, like the Jumna; but the latter I believe to be the case. I apprehend that three considerable streams which, like the Jumna, originate from the south faces of the Himālaya, in the districts of Barasa, Leulowari, and Deodara Kowarra, join to form the Tonse; and it receives a considerable accession of water from the Paber river, which I imagine to be equal in size to any of the three above-mentioned feeders. Respecting them, I have at present only native information to guide me, but of the Paber, I can speak with more confidence; for when, in June, 1816, I penetrated within the Himālaya by the course of the Setlej, I found that the north bases of many of the snowy peaks seen from the plains of Hindustan, were washed by that river. Its course, in the province of Kunaur, in latitude 31° 31′, and longitude 78° 18′; being from east 25 S. to 25 to the N. of west. In this position, the Setlej is bounded both to the N. and S. by high and rugged snowy mountains, from which many torrents descend, and increase its bulk. Leaving the left bank and bed of the river, I ascended the snowy range, of which it washes the north base, and crossed over it on the 21st June, 1816, at 40 minutes past 11 o'clock in the forenoon, during a heavy fall of snow, being the first European who effected a passage over the grand Himālaya ridge in that direction.

On surmounting the crest of the pass, I found that the Indravatī river, which is a principal branch of the Paber, originated from the snows, on which I descended on the SW. or hither side of the ridge; and I followed its channel to the place where it joins the Paber, which river must have its beginning, in like manner, on the same side of the ridge, as I was informed by the people of the country it had, and I am nearly certain it is the case; and it is most probable that all the streams which form the Tonse do, in like manner, descend from the SW. side of the fronting snowy range, the NE. base of which is washed by the Setlej, as above-mentioned.

However, I intend to explore the sources of the Tonse, as well as of the Setlej and Jāhnavī rivers. But to return to the Jumna.

The route from its confluence with the Tonse in the Dūn is thus: to Calsī four miles, a large village immediately within the mountains of Jaunsar, of which district it is esteemed the capital. It is situated between two high and steep mountains, and on the Omla, a small river which joins the Jumna. Calsī is a place of some little trade, as the people of the neighbouring mountains bring to it their productions, and exchange them for cash to pay their rents, and a very small quantity of the produce of the plains. On the march, the Jumna is forded above its confluence with the Tonse. Carriage cattle may go to Calsī, but further within the mountains every article is carried on men's backs. Latitude of Calsī, 30° 31′ 24″.

Six thousand paces of exceedingly steep ascent of the mountain on left bank of the Omla; 2600 paces easier to the village of Khuny on the ridge; remainder, along the mountain's side, with occasional ascent and descents to the foot of the peak of Birat, which rises conically above the ridge; 1800 paces of the steep ascent up it to the fort, which is a small double enclosure. It was abandoned by the Gorc'ha garrison on the approach of a force under Col. Carpenter.

The height of Birat above Seharanpur (which is visible from it) is 6508 feet; it commands a noble view of the snowy mountains and the various intermediate ranges, as well as of the Dūn valley, and the plains on both sides of the Jumna.

Invalids from the plains requiring a change of climate may find it at Birat. In the winter the fort is almost buried in snow, which remains in shady places, and on the northern side of the peak till the beginning of April; but snow seldom falls later than the last week of March, at which season, while I was in the fort, there was a shower which covered the ground to the depth of two inches: the peak is a bare slaty rock, with some quartz intermixed.

March 29, 1817.—Narrow path along the mountain's side; then a steep descent of 2 m. 1 f. to Murlang, a small village in a glen on the Silgad rivulet, which falls into the Jumna three miles to the E. No grain here. Latitude observed 30° 36′ 53″. Thermometer at noon 78°. It was yesterday at noon at Birat, 50°.

Proceed 2½ miles down the bed of the Silgad to the Jumna; then leave it, and cross a ridge, and go up the bed of the Jumna to the confluence of the Cunti river, which joins it from the Keinah peak to the west. That river is about 60 feet wide, and 1½ and 2 feet deep. The Jumna is 90 feet wide, 3 to 5 feet deep, rapid, and not fordable. The rest of the path is a long ascent of the mountain, above the right bank of the Jumna to Cot'ha, a village of 10 houses, about 3000 feet above the level of the river. A fatiguing march; heavy rain. No grain here.

The path lies generally along the side of the mountain, with occasional strong ascents and descents; 1 m. 5 f. of very steep descent into a dell, the rest lighter descent, flat and ascent from a rivulet to Lak'ha Mandal, on the right bank of the Jumna, and about 300 feet above it.

Lak'ha Mandal is a place of some celebrity in Hindu story, as having been one of the temporary residences of the Pandus; and tradition says, that formerly there were a great number of statues and temples here, but I imagine the greater part to have been buried by the slip of the side of the mountain at the foot of which it is situated. Several pieces of cornices, entablatures, and other ornamental fragments of buildings, are seen projecting above the soil, which buries the remainder; they are of black stone, and the carving of the ornaments is very well executed. There are also two statues of Bhim and Arjun of the size of life, which are half buried in the soil; and a prodigious number of small idols are deposited in a little temple, which is the only one now remaining, and which does not appear to be of any remote antiquity. The ignorant Brahmin could give no account of the builder; he declared, as they all do, when consulted on such subjects, that it is not of human workmanship, but was built by Bhim countless ages ago.

It does not appear that pilgrims now resort here; the place is nearly desolate; it is surrounded by high rocky peaks, and may have been chosen as a fit seat for gloomy and recluse superstition.

Within the temple there is a large slab of blue stone inscribed with Hindu characters; I cleaned it, and took off a reversed impression as well as circumstances would allow, and sent it to Col. Mackenzie. Latitude of Lak'ha Mandal, 30° 43′ 24″.

Gradual descent 1½ mile to the Ricnar river, which is the boundary between Sirmor and the Rewaen district of Gurhwal. It has a course of about 10 miles from the NW. and joins the Jumna here. From the river, a very strong ascent of 1¼ mile up the mountain to a crest called Genda Ghat; there obliquing to Bancauli, a village of 20 houses, with a temple; it is on the mountain's side, and about 3000 feet above the Jumna. No grain to be had here as at other places. I planted potatoes. Rainy weather. No latitude.

To the bed of the Jumna 3 m. 3 f. mostly oblique descent, though steep in some places above the right bank of the river. Here are very high and steep precipices, from which large blocks of granite have fallen into the bed of the river, which forces its way through and over those obstructions with much violence and noise. After passing over the rocks by the river side for half a mile, we leave it, and climb the right bank by an exceedingly steep ascent to the Tocm Ghati, which overhangs the stream, and is about 1000 feet above it. Hence descend a mile to the Camaulda river; cross it on trunks of trees laid across, a little above its junction with the Jumna.

The Camaulda is the largest river which the Jumna receives above the confluence of the Tonse; its course is from N. 10° west, down the Rāma Serai district, which is a small valley, and is reported to be in some places a mile wide, but it is now overrun with *jungles*, full of wild beasts. The Camaulda, now swollen by the rain, is about 70 feet wide and 2½ feet deep, and very rapid. Immediately on crossing it, the country up the Jumna assumes a more pleasing appearance; the mountains which bound it, though very lofty, do not rise so abruptly, and several small villages are seen on their lower slopes. On the right bank of the river, there is a slip of level ground from 300 to 500 yards wide. The summits of the mountains are covered by cedars and other pines, and the snow yet lies on them.

Proceed by the river side to Paunti, a village of 20 houses, pleasantly situated about 400 feet above the Jumna. The march was long and fatiguing, as it rained the whole way; the loaded people did not arrive till after dark. At this village I got supplies of grain. The country I have passed through from Calsī is nearly deserted, on account of famine caused by the crops of last year having been destroyed by the hail in October. Aware of this circumstance, I have brought grain with me from Calsī, and subsisted my followers with it. Latitude of Paunti, 30° 48′ 08″.

Two and a quarter miles parallel to the Jumna, and descend to its bed, where the stream from the Banaul glen joins it. Leave the Jumna, and proceed three miles NW. up the Banaul river. Then ascend the south face of the mountain to Gira, a village of

10 large houses pleasantly situated, and sheltered from the northern blasts. This district of Banaul is about seven miles in length; the NW. end is closed by a high rocky mountain, where the stream arises, which waters the bottom of the glen. Several villages are seen placed in advantageous situations on the sides of the mountains, the soil of which is fertile; wood, water, and grain are abundant.

As I learned that much snow yet remained on my route forward, I halted here some days, to give it time to melt, and to refresh my people who were harassed by the journey from Calsī, for it had rained every day, and they had been sparingly and ill fed, and also to take the rates of my chronometers. I took two immersions of Jupiter's satellites: Latitude of Gīra, 30° 52′ 08″.

Gīra to Thanno; total distance eight miles. Down the north side of the glen, and pass the villages of Bisat and Devah to Dakiat, a large village, 4 m. 6 f. Proceed parallel to the Jumna, but above it, 1 m. 6 f. and descend to the Badal river, which comes from a glen similar to that of Banal, but is longer, and contains more and larger villages.

The river joins the Jumna here; it comes from the Cēdārn Cānta, a large mountain covered with snow, and its course is from N. 15° west; breadth about 40 feet; depth 1½ and 2 feet. Proceed 1¼ mile further to Thanno, a small village, 400 feet above the right bank of the Jumna.

The road to day chiefly on a gradual descent; path good and pleasant. The Jumnotri snowy peaks seen up the river, have a noble appearance; the eastern peak bears 56° 17′ N.E.; its altitude 8° 16′. Thanno appears to be 4083 feet above the level of Seharanpur. Latitude observed 30° 49′ 12″.

Thanno to Catnaur; total distance 4 m. 2 f. Steep descent to the Jumna, and cross it on a sangha, which consists of three small spars and some twigs bound together, and laid across in the manner of a hurdle. The sangha is in two portions, being laid from rock to rock; one is nine paces in length, and the other seven, the breadth of the river being about 40 feet; but it is deep, being confined between the rocks, through which it falls like a cataract. The water nearly touches the bridge, which is a bad one. Some of my goats fell through it, and were drowned. Above this place, the bed of the Jumna is much inclined; the stream bounds from rock to rock, and for the most part is a series of small cataracts.

A mile beyond the sangha, cross the S'ilba, a small river from the glen of that name, and proceed to Catnaur, a small village 500 feet above the left bank of the Jumna. Up the S'ilba glen is a convenient pass over the ridge, which separates the Ganges and Jumna.

The path to day chiefly ascent and descent, and very rough and steep in most places; and hence forward the features of the *mountains* bear a harsher appearance, there being generally mural precipices rising from the bed of the Jumna to the height of 1500 to 2000 feet, either on one side or the other. The summits of the mountains all round are deep in snow. A stream from a peak called Dallia Cursu joins the Jumna here from the SE. Latitude observed 30° 51′ 35″.

As no grain was to be had here, I was obliged to march in the afternoon to a very large village called Pāli, situated up a wild glen; this was a good deal out of my route. The inhabitants of Pāli and the neighbouring villages have been noted for a rebellious spirit against both the Gur'hwal and Gorc'ha governments. They had cut off several parties of the Raja's troops, and surprised and destroyed a complete company of Gorc'has several years ago, for which they were punished by a force sent against them under the brave chief B'hacti T'hapa. On my arrival, they refused to sell me any supplies, and I expected to have had trouble. However, towards evening, we came to a better understanding, and I got abundance of grain. The village consists of about 50 large houses; the inhabitants are stout and hard featured, and the women generally have light complexions, and agreeable countenances. In the morning I went down the glen 1¼ mile, and then along the right bank of the Jumna, but high above it, by a difficult and very unpleasant pathway overhanging it. In one place I was obliged to go with great caution, and bare footed, for a false step would be fatal. The precipices on the opposite side of the river are quite perpendicular, and on this exceedingly steep. After passing the worst part, descend to Oj'ha Ghur, a hamlet of three huts only, in a dismal situation, at the feet of steep and lofty cliffs, the rocks hurled from which by the earthquake of 1803, buried a small fort and village which once stood here. Dreadful mementos are seen in these mountains of the effects of that catastrophe. Under Oj'ha Ghur, a stream falls into the Jumna, and several cataracts are seen falling among the surrounding precipices. There are some hot springs at the bed of the Jumna which is 400 feet below the hamlet. Latitude observed 30° 54′ 47″.

Oj'ha Gur to Rānā; total distance 4 m. 5 f. In paces 91·815. 2655 paces along the mountain's side, and descent to the Jumna. Cross it on a sangha of two small spars; its length 20 feet; breadth about 2¼ feet. The river rushes with great violence under the sangha, and nearly touches it. The general breadth of the stream is greater, but it is here confined between two rocks.

1200 paces by the margin of the river; the rest, for the most part, ascent, and in some places very steep and rugged.

Rānā is a small village of 15 houses, about 800 feet above the left bank of the river on the slope of the mountain; the general lower line of snow on it does not appear to be more than 1000 feet above the village. The opposite bank of the river is composed of yellow *granite precipices* rising murally from the stream to the height of about 2500 feet or more. The courses of the rock are disposed almost horizontally as high as 1000 feet above the river; but, towards the summits, they appear to incline in an angle of about 35°, the apex being to the SW. Heavy storms of hail and thunder.

Rānā to Bannasa; distance 7839 paces. Ascents and descents to the small village of Bārī 2356 paces; 684 paces further descent to the Burha Ganga river, which has a course of about eight miles from the snows to the right; it is in two streams, each eight paces wide, and 18 inches deep, and joins the Jumna; 1480 paces of exceedingly steep ascent; the remainder, ascents and descents, and difficult road. Cross the Jumna on a sangha, and also the Bannasa river, which is about two-thirds of its size, and joins it here. Ascent to Bannasa, a small village, at the foot of a rocky mountain, a fall from which last year destroyed half the village. Angle of altitude of the mountain, 40° 55′. Among the cliffs and on the summit, I observed with a telescope many of a species of animal peculiar to these elevated regions; it is called Pheir, and as a mountaineer in my service succeeded, after many toilsome chases, in shooting one of them, I can give a description of its dimensions.

	Feet.	In.
Length from the tip of the nose to the end of the tail, the length of the face 11 inches, and of the tail, 3 inches only	5	0
Height from shoulder to toe	3	2½
Girth at the chest	2	11¼
Girth at the loins	2	4

Length of the hair at the shoulders, eight inches, but on the other parts of the body it is short. I preserved the skin and the bones of the head and horns, and presented them to the Most Noble the Governor-General, who, I believe, sent them to Sir Joseph Banks.

The face of the animal, which was a male, resembles that of the Nil Gao. The horns are large, the lower part of them stands nearly erect from the forehead, but the upper half bends backward. The hoofs, cloven. The colour, that of a camel or lion, and the long hair about the shoulders and neck somewhat resembles a lion's mane. The flesh appeared coarse, and an unpleasant musky smell exhaled from it. The Hindustanis would not touch it, but the Gorc'ha Sipāhīs, and mountaineer Coolies ate it with avidity. It is remarkable that those people will not eat mutton. The Pheir is a gregarious animal, and appears to subsist on the short herbage at the edge of the snow. The chase of it in its haunts on the cliffs and precipices is most difficult and dangerous; but in the depth of winter when the snow drives them down to the villages, the people hunt and kill them more easily.

In this neighbourhood springs of hot water are very numerous; they are seen bubbling up among the rocks in various places near the rivers. The heat of the water is too great to bear the hand in it for many moments; but having broken my long scaled thermometer, I could not ascertain its precise temperature. The water has little if any taste. About half a mile above its junction with the Jumna, the Bannāsa river falls from a precipice of yellow and rose-coloured granite, of 80 or 90 feet high, in a noble cascade. The breadth of the stream is about 15 feet, and it falls, with much noise, into a deep basin, which it has worn in the rock.

The stream is caused by the melting of the snows on the heights above.

From the village, two of the Jumnotri peaks appear towering above the clouds with sublime effect. Angle of altitude (taken by reflection in mercury) of the east peak, 15° 34′ 45″, of the west, 17° 10′ 10″.

Bannāsa.—Longitude of Bannāsa, 5° 13′ 47·9″.

The beginning of twilight made the observation not so good as it would have otherwise been. Lat. observed, 30° 55′ 50″.

This is not a good latitude. The weather was cloudy and stormy, with showers of sleet.

Bannāsa to Cursali; thermometer at sunrise, 33°.

Descend to the Jumna, and cross it on a plank 12½ feet long, and again on a plank of 10 feet; depth of the water 2½ feet; beds of frozen snow extend to the margin of the stream. A most laborious and steep ascent of 675 paces, whence gradually descend, and cross the Jumna on a small sangha, where it receives the Imri rivulet from the snow, whence it originates, about 1½ mile to the end. It is less than the Jumna, which is now reduced to the rank of a rivulet. Strong ascent to the village of Cursali. Total distance 4978 paces.

Stormy weather and very cold; driving showers of sleet and rain; path bad and slippery.

The village of Cursali contains about 25 substantial houses, and is situated at the immediate feet of the Jumnotrī snowy peaks; but they are not visible, as the near and steep part of the base obstructs the view. The situation is very peculiar, and one would hardly suppose that people should choose to live in such a remote and cold place. It is the latter end of April, and yet daily slight showers of snow fall, and the remains of drifts yet lie in shaded places in the village. By the sides of the Imri and Jumna, there are several spots of flat ground on which the inhabitants cultivate grain enough for their subsistence. To the west, north, and east, this little secluded place is bounded by the lofty cliffs of the Himālaya; and to the south it is sheltered by a mountain, the north face of which is not so steep, and it is clothed with trees. All those are at present deep in snow, which reaches down to the level of the two streams; yet I found the place by no means an uncomfortable abode, for the heights near it shelter it from the violence of the winds. The sun is pleasantly warm in the middle of the day, and the progress of vegetation is rapid in proportion to the length of the winter. The rocky and snowy defile called Jumnotri, where the Jumna originates, is seen in the direction of N. 42° east. Distant three miles. Latitude of Cursali, 30° 57′ 19″.

During three days I attempted to get some sets of lunar distances, and also transits of the moon over the meridian, but was constantly prevented by clouds from doing any thing satisfactorily.

Cursali to Jumnotri. Flat along the village fields: here climb a steep rocky corner above the river's bed. Jumnotri nearly 41° 30′. Chia mountain, over which there is a pass to Suc'hi on the Ganges, practicable in the rains (at present, it is blocked up by deep snow).

Steep descent through snow 1 to 5 feet deep; then flat.

Fields. Slight acclivity; snow patches. Abundance of pheasants here, chiefly of the kind called Monal.

Rough and rocky; descend to the Jumna, which in several places flows under beds of snow 25 or 30 feet thick. An overhanging precipice to the right. A torrent called the Bandiali, half the size of the Jumna, joins it from a cleft in the rock, and is the first tribute it receives. The path to this station entirely through snow; cross the river twice, once on the stones, and once on a snow arch.

At Bhairo Ghati. The crest of one of the steepest ascents (for its length) I ever saw; it is entirely up the snow, in which we cut steps with p'haoras (spades) to facilitate our passage. There is here a place dedicated to Bhairo Lal, who is esteemed to be the Janitor of Jumnotri and Gangotri. It is nothing more than a low building (if it may be so called) of three feet high, containing some small iron tridents. I hung a new English silver coin by a copper ring on one of them.

Exceedingly steep descent to the Jumna by steps cut in the snow. A cascade of the stream cuts through the snow, and falls from a rock of the height of about 50 feet.

Stiff ascent up the snow bed, which conceals the river. Except here, where the stream is visible for a few yards through a hole in the snow, the snow bed is about 100 yards wide, and bounded by high precipices, from which masses of rock of 40 feet in length have recently fallen.

River as before under the snow: here it appears through a deep hole falling in a cascade from the rock below the snow. Rocks on both sides, those to the right cased with ice.

At Jumnotri, the snow which covers and conceals the stream is about 60 yards wide, and is bounded to the right and left by mural precipices of granite; it is 40 feet 5¼ inches thick, and has *fallen from* the precipices above. In front, at the distance of about 500 yards, part of the base of the great Jumnotri mountain rises abruptly cased in snow and ice, and shutting up and totally terminating the head of this defile, in which the Jumna originates. I was able to measure the thickness of the bed of snow over the stream very exactly by means of a plumb line let down through one of the holes in it, which are caused by the steam of a great number of boiling springs which are at the border of the Jumna. The snow is very solid and hard frozen; but we found means to descend through it to the Jumna by an exceedingly steep and narrow dark hole made by the steam, and witnessed a very extraordinary scene, for which I was indebted to the earliness of the season, and unusual quantity of snow which has fallen this year. When I got footing at the stream (here only a large pace wide), it was some time before I could discern any thing, on account of the darkness of the place, made more so by the thick steam; but having some white lights with me, I fired them, and by their glare was able to see and admire the curious domes of snow overhead; these are caused by the hot steam melting the snow over it. Some of these excavations are very spacious, resembling vaulted roofs of marble; and the snow as it melts falls in showers, like heavy rain, to the stream, which appears to owe its origin in a great measure to these supplies. Having only a short scaled thermometer with me, I could not ascertain the precise heat of the spring, but it was too hot to bear the finger in for more than two seconds, and must be near the boiling point. Rice boiled in it but imperfectly. The range of springs is very extensive, but I could not visit them all, as the rest are in dark recesses and snow caverns. The water of them rises up with great ebullition through crevices of the granite rock, and deposits a ferruginous sediment, of which I collected some: it is tasteless, and I did not perceive any peculiar smell.

From near this place, the line of the course of the Jumna is perceptible downward to near Lak'ha Mandal, and is 55° 40′ SW. It will be seen by the notes that from the place called Bhaira Ghati, the bed of the river is overlaid with snow to the depth of from 15 to 40 feet, except at one or two places, where it shows itself through deep holes in the snow.

The snow bed is bounded to the right and left by mural precipices of light coloured granite: on some ledges there is a sprinkling of soil where the b'hojpatra bushes grow. The end of this dell or defile is closed, as before observed, by part of the base of the great snowy mountain of Jumnotri, and which is visible from the plains. The altitude of the part of the mountain visible is 29° 48′; but higher parts are concealed by the lower and nearer. The face of the mountain, which is visible to the height of about 4000 feet, is entirely cased in snow and ice, and very steep. The foot of the base is distant from the hot springs about 500 yards, and immediately where the ascent becomes abrupt, a small rill is seen falling from a rock which projects from the snow; it is about three feet wide, and shallow, being only a shower of spray produced by the snow now thawing in the sun's rays at noon. Above that no water whatever is seen; if there were any, it would be visible, as the whole base of the mountain is exposed to view, directly in front; consequently the above rill is the most remote source of the Jumna. At the present season it was not possible to go to it, as the snow bed was further on impassable, being intersected by rents and chasms caused by the falling in of the snow, as it melts by the steam of the boiling springs below it.

Here then is the head of the Jumna on the SW. side of the grand Himālaya ridge, differing from the Ganges, inasmuch as that river has the upper part of its course within the Himālaya, flowing from the south of east to the north of west; and it is only from Suc'hi, where it pierces through the Himālaya, that it assumes a course of about S. 20° W.

The fall of the Jumna from Jumnotri to the Dūn is very considerable. I regret I had not a good barometer to ascertain the height of Jumnotri. I had with me an empty country-made barometer tube with which I endeavoured to gain an approximate idea on the subject. Having warmed and well dried the tube, I filled it gradually with mercury, driving out such air bubbles as were visible, and inverted it in a deep cup of quicksilver, taking care not to remove my finger from the orifice till the lower end of the tube was fairly below the surface of the quicksilver; the tube was kept in an erect position by means of a plumb line.

The length of the column was 20·40 inches, which, corrected for temperature, gives 10,483 feet for the height of Jumnotri above the sea, taking 30·04 inches for the level of the sea.

The above is only a rude experiment, but I had not the means of making a better; the length of the column may be depended on to the 20th part of an inch, I think, but the probable impurity of the mercury may cause an error of 200, or, perhaps, 300 feet.

Near noon, I took a short set of circum-meridional altitudes of the sun for the latitude. Mean latitude of the hot springs of Jumnotri, 30° 58′ 52·1″.

The latitude of the small fall or rill, which may more properly be called the head of the Jumna, will be 30° 59′ 06″.

April 21.—Having finished my observations by two o'clock, I set out to return; the heat of the sun had then begun to melt the snow on the cliffs on both sides, and many rocks and lumps of snow were falling down: this obliged us to run with all speed down the snow bed to get out of the way of these missiles. Several of the people had narrow escapes from the falling fragments, but no one was struck.

The inhabitants of Cursali say, that it is 17 years since they had so severe a winter as the last. At Jumnotri, the inclination of the granite rock is from 43° to 45° from the horizon; the apex being to the SW. or towards the plains.

As the season was not sufficiently advanced to allow of my passing to the Ganges by the Chīā or Cīlsaum mountains, both of which are at present impassable from the depth of snow on them, I returned to Catnaur, and going up the Shialba glen, crossed the ridge, which divides the two rivers at the Jackeni Ghāt, and descended by Bauna to Barahat, from whence I proceeded up the Ganges to Reital, and continued my route beyond Gangotrī, as before mentioned.

I shortly hope to be able to present to the Society the result of my trigonometrical operations to determine the heights and positions of all the peaks of the Himālaya visible from Seharanpur, and also an account of the sources of the Tonse and Jāhnavī rivers, and of the upper part of the course of the Setlej.

Article III.

On a New Lead Ore. By H. I. Brooke, Esq. FRS. & FLS.

(To the Editor of the *Annals of Philosophy.*)

SIR, *July* 6, 1822.

The third volume of the Edinburgh Philosophical Journal contains a notice from me of three varieties of lead ore which had not been before accurately described. I have now to add a fourth, of which only a very slight account has been given by Mr. Sowerby in the third volume of his Brit. Min. p. 5, under the name of blue carbonate of copper.

It is only within a few days that I have had an opportunity of examining this substance.

The specimens I have seen, as well as that figured by Mr. Sowerby, were found at Wanloch Head or Lead Hills.

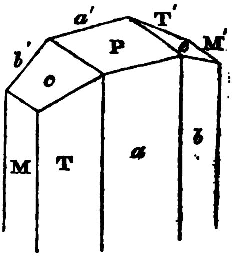
The facility with which this species may be cleaved, the brilliancy of the cleavage planes, and the angle at which those planes incline to each other were indications that the substance was not carbonate of copper; and it appears on examination to be a compound of sulphate of lead and hydrate of copper, and may be denominated *cupreous sulphate of lead.*

The colour resembles the brightest specimens of blue carbonate of copper.

Specific gravity about 5·3, but as the specimens I possess are not perfectly free from included particles of carbonate of lead and of cupreous sulphato-carbonate of lead, it is probable that the specific gravity of more perfect specimens may differ in a small degree from that which I have given. A fragment of 1·4 grain, which is more transparent than the general mass of the substance, has indicated a specific gravity of 5·43.

It scratches sulphate, but is scratched by carbonate of lead.

For the purpose of enabling me to describe the crystalline form more accurately than I could have done from my own specimens, Mr. Sowerby has favoured me with a couple of small crystals whose form is rudely represented by the annexed hastily drawn figure.



The cleavages are parallel to the planes M and T. That parallel to M may be effected almost by pressure between the fingers. I have not observed any transverse cleavage, but as the plane P is at right angles to M and T, and as the plane M does not meet the planes *b′* and T at the same angle, the primary form may be regarded as a right prism whose base is an oblique angled parallelogram.

The measurements are as follows, the letters *b′*, *a′*, T′, and M′, being placed above the edges of the planes to which they relate:

M on T	102°	45′
b′	104	50
c	120	30
P	90	0
T on *a*	161	30
M′ on *b*	104	50
T′	102	45
P on *a*, or *a′*	90	0

If we suppose the plane *b* to result from a decrement by one row on the acute lateral edge of the prism, the terminal edge of the plane M would be to that of plane T nearly as 11 to 23, and if the planes *c* and *c′* are produced by a decrement by one row on the terminal edges, the height of the prism will be to the greater terminal edge as 13 to 23 nearly.

The specimen I possess is so small, and so little of it is perfectly pure, that I have not been able to submit more than a few grains to analysis. The result of this has given the following proportions of the constituent parts of the mineral:

Sulphate of lead	75·4
Oxide of copper	18·0
Loss by heating	4·7
	98·1

As there was not any effervescence perceptible during the solution of the mineral in sulphuric acid, the loss by heating must have been occasioned by the loss of water only; and if we assume as the equivalent for sulphate of lead, 190 = 1 oxide of lead + 1 sulphuric acid, and for that of hydrate of copper, 122·5 = 1 peroxide of copper + 2 water, the substance would approach very nearly to a definite compound of

2 atoms sulphate of lead equivalent to..	75·5
1 atom hydrate of copper............	24·4
	99·9

H. I. Brooke.

Article IV.

Account of Dr. Hare's improved Deflagrator, and of the Fusion of Charcoal and other Phenomena produced by it.

A description of the instruments invented by Dr. Hare, of Philadelphia, and named, the one a Calorimetor, and the other a Deflagrator, has been given at p. 176, vol. xiv. and p. 329, vol. i. New Series, of this journal. A correspondence between Dr. Hare and Dr. Silliman will appear in the American Journal of Science, containing the description of a new Deflagrator, and of various interesting phenomena presented by those instruments, of which the following is an account drawn up from the letters forwarded to the Editor by Dr. Hare.

From various considerations Dr. Hare was induced to construct an instrument consisting of zinc plates surrounded by copper cases. The zinc plates were seven inches by three, and the copper cases were of such a size as to receive them much in the manner of Wollaston's construction. "There was, however," says Dr. H. "this apparently slight but really important difference, that the cases employed by me were open at top and bottom instead of opposing the edges of the zinc laterally, as in Wollaston's. One hundred galvanic pairs thus made were suspended to two beams, each holding 50. Between each case, a piece of pasteboard soaked in shell lac varnish was interposed, so that the whole constituted a compact mass, into which a fluid could not enter, unless through the interstices purposely preserved between the copper and the zinc." This apparatus was equally powerful with the original deflagrator, yet its oxidizable surface was not of more than half the extent, and it was comprised in one-eighth part the space.

In this construction of apparatus, where two or more beams of plates were used, they were fixed side by side in a frame, and connected one with another as in the common voltaic instrument. Then troughs without partitions, one for each beam, were placed on a platform beneath them, and being filled with water or acid, were raised by levers and a treadle under the platform until the plates were immersed. By a series of 250 pair, barytes was deflagrated, and the platina which supported it destroyed like pasteboard before an incandescent iron. Platina wire, 3-16ths of an inch in thickness, was made to flow like water. Iron, of like dimensions, burned explosively. Mercury was deflagrated by connecting two vessels containing the metal with the poles of the instrument, and then letting a stream run from one into the other through a small orifice.

"Probably the most useful mode of applying such instruments to analysis would be to expose substances in carbon to the discharge in vacuo. I observed that after iron and charcoal were ignited between the poles during a few seconds under an exhausted receiver, on admitting the air, a flash took place, and a yellowish red fume appeared which condensed on the glass. It would seem the iron was volatilized, and that the admission of air oxidized the vapour."

An instrument of this kind produces torture when applied for a short time to the back of the hand, and is most sensible over any of the most turgid veins, where the skin is tender: there is very little difference in the sensation with a charge of water or of acid, but the positive pole is most capable of producing pain. The shock is not greater in any sensible degree at the moment of immersion than afterwards. The instrument has the power of affecting a very delicate electrometer. A magnetic needle was very powerfully disturbed by the deflagrator under all its forms.

In a letter from Dr. Silliman to Dr. Hare is then described the incompatibility which he had discovered of the voltaic batteries, and the instruments of Dr. Hare when used in connexion. The instrument used was the deflagrator of 80 coils, and when placed in one common recipient or each coil in a separate jar, the effects were the same. The deflagrator being connected by its proper poles with a galvanic battery of 300 pair four inch plates interposed between the two rows of the deflagrator of 40 coils each, lost all its power, and the effect produced was very much inferior to that of the battery alone; for in fact the spark was hardly perceptible. The chemical powers of the battery were also destroyed; the 300 pairs usually decomposed water, salts, &c. with decisive energy, but now hardly produced a bubble of gas, or affected dilute infusion of red cabbage. The power of giving a shock was also destroyed. When the coils were raised out of the fluid and suspended in the air, they acted merely as conductors of the power of the common battery only a little diminishing it. These experiments were made with different combinations from 620 pairs down to 20, and uniformly produced an almost entire suspension of the power of both instruments.

In one experiment, 25 pairs of zinc and copper plates, six *inches square,* connected by slips of copper, and suspended from a beam, were immersed in a trough without partitions containing an acid liquor and the deflagrator then connected, its power was completely destroyed; a similar result was obtained when 50 pairs of Wollaston's plates were used: the object in these experiments was to ascertain whether a battery in which the arrangement of metals was similar to that in the deflagrator would produce the same result as the common battery, which was the case. In most of the experiments, the connexion of the poles was occasionally reversed. This circumstance, however, made no difference in the results; a feeble spark was obtained as before. "Every thing tended to countenance the opinion *that the interposition of the common galvanic battery operated simply as an impediment—that it was completely inert in relation to the deflagrator, and the deflagrator in relation to it—that the power of neither would pass through the other, and consequently that each was to be regarded, with respect to the other, simply as so much interposed matter constituting a conductor more or less imperfect.*" This was proved by diminishing the number of interposed plates; when there were 20, the power of the deflagrator passed freely, but diminished. As the number was made smaller, the power increased, and when one pair only remained, there was no perceptible impediment to the power of the deflagrator.

In another letter, Dr. Silliman relates the phenomena of the fusion of charcoal; having been excited to a close observation of what took place when charcoal was subjected to the power of these instruments, by some observations of Dr. Hare.* The pieces of charcoal were prepared by igniting mahogany, buried beneath white siliceous sand in a crucible; they were about half an inch in diameter, and from 1¼ inch to 3 inches in length; they were tapered to a point, and the cylindrical ends placed in sockets connected with the flexible leaden tubes which form the polar termination of the series.

"The metallic coils of the deflagrator being immersed, on bringing the charcoal points into contact, and then withdrawing them a little, the most intense ignition took place; and I was surprised to observe that the charcoal point of *the positive pole* instantly *shot out* in the direction of the longer axis, and thus grew rapidly in length; it usually increased from the tenth to the eighth of an inch, and in some instances attained nearly one-fourth of an inch in length before it broke off and fell. Yesterday and to-day, I have carefully repeated these experiments, and in no instance has this shoot from the positive pole failed to appear. It continues to increase rapidly, as long as the contiguous points of charcoal are held with such care that they do not strike against each other. When they impinge with a slight shock, then the projecting shoot or knob breaks off and falls, and is instantly succeeded by another. The form of the projecting shoot is sometimes cylindrical, but more generally it is that of a knob connected with the main piece of charcoal by a slender neck, much resembling some stalagmites. It is always a clear addition to the *length* of the charcoal, which does not suffer any waste except on the parts *laterally* contiguous to the projecting point.

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* Vol. i. (New Series) p. 333, of *Annals of Philosophy*.

"The charcoal of the negative pole in the mean time undergoes a change precisely the reverse. Its point instantly disappears, and a crater-shaped cavity appears in its place; it suffers a rapid diminution in the direction of its length, and immediately under the projecting and increasing point of the positive pole; but it is not diminished, or very little, on the parts laterally contiguous. If the point of the positive pole be moved over various parts of the contiguous negative charcoal, it produces a crater-shaped cavity over every place where it rests, for an instant. In every repetition of the experiment (and the repetitions have been numerous), this result has invariably occurred. *It appears as if the matter* at the point of the negative pole was actually ransferred *to the positive, and that the accumulation there is produced by a current flowing from the negative to the positive, or at least by an attraction exerted in that direction, and not in the other.* It does not appear easy to reconcile this fact with any electrical or igneous theory.

"In order to ascertain whether the projection of the charcoal at the positive pole was caused by an actual transfer of carbon from the negative, a piece of metal was substituted for the charcoal at the negative pole, and when the two were brought into contact, the charcoal point of the positive pole remained unaltered in form, although a little shortened by the combustion. The experiments with the two charcoal points were varied by transferring, that at the positive end (and on which a projection was already formed) to the opposite pole, and that at the negative, and in which a corresponding cavity appeared to the positive.

"The result was that the cavity now placed at the positive pole disappeared, and was immediately seen at the negative; while the projection now placed at the negative pole was transferred to the positive. These experiments were several times repeated, and uniformly with the same result. They seem to leave no doubt *that there is a current from the negative to the positive pole, and that carbon is actually transferred by it in that direction;*[*] if transferred, it must probably be in the state of vapour, since it passes through the ignited arch of flame, which is formed when the points are withdrawn a little distance; when it arrives at the positive pole, it there concretes in a fluid, or at least in a soft or 'pasty' state.

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[*] Those who would contend for a current in the opposite direction would probably say, that the projecting point of the positive pole is formed from the carbon contiguous on the sides, and that the stream of heat burns the cavity in the opposite pole; *in either way a current is proved.*

"But the most interesting thing remains yet to be stated. On examining with a magnifier the projecting point of the positive pole, it exhibited decisive indications of having undergone *a real fusion.*

"The projecting point or knob was completely different from the charcoal beneath. Its form was that of a collection of small spheres aggregated, exhibiting perfectly what is called in the descriptive language of mineralogy botryoidal or mamillary concretions. Its surface was smooth and glossy, as if covered with a varnish; the lustre was metallic, the colour inclining to grey, exhibiting sometimes iridescent hues, and it had entirely lost the fibrous structure. In short, in colour, lustre, and form, the fused charcoal bore the most striking resemblance to many of the beautiful stalactitical and botryoidal specimens of the brown hematite. The pores of the charcoal had all disappeared, and the matter had become sensibly harder and heavier.

"I repeated the experiments until I collected a considerable quantity of these fused masses; when they were placed contiguously upon some dark surface, with some pieces of charcoal near them, they appeared when seen through a magnifier so entirely different from the charcoal, that they would never have been suspected to have had any connexion with it, had it not been that occasionally some fibres of the charcoal adhered to the melted masses. The melted and unmelted charcoal differ nearly as much in their appearance as pumice stone and obsidian, and *quite* as much as common stones do, from volcanic scoriæ, excepting only in the article of colour. It is to be understood that the examination is, in every instance, made by means of a good magnifier, and under the direct light of the sun's rays, as the differences are scarcely perceptible to the naked eye, especially in an obscure light. The portions of melted charcoal are so decidedly heavier than the unmelted, that when fragments of the two of a similar size are placed contiguously, the latter may be readily blown away by the breath, while the former will remain behind; and when the vessel containing the pieces is inclined, the melted pieces will roll with momentum from one side to the other in a manner very similar to metallic substances, while the fragments of charcoal will either not move, or move very tardily.

"It should be observed that during the ignition of the charcoal points, there is a peculiar odour somewhat resembling electricity, and a white fume rises perpendicularly, forming a well defined line above the charcoal. There was also a distinct snap or crackling when the two points were first brought together.

"Wishing to ascertain whether the alkali present in the charcoal had any effect in promoting the fusion, some pieces of prepared charcoal were thoroughly boiled in water, and were then again exposed to a strong heat in a furnace beneath sand in a crucible. These pieces when connected in the circuit exhibited the same appearances as the others, and proved equally fusible.

"Without destroying cabinet specimens, I could procure no diamond slivers, and have not, therefore, attempted the fusion of the diamond, which must be left to another opportunity. Our circle of fusible bodies so much enlarged by the use of your instruments is now so nearly complete that it would be very desirable to fill the only remaining niche, namely, that occupied by plumbago, anthracite, and the diamond.

"I do not suppose that those who repeat these experiments will succeed with the common galvanic apparatus. I deem it indispensable that they be performed with the *deflagrator*, and with one equal in power to mine."

Dr. Hare's views of the phenomena of voltaic electricity have at different times been stated in the *Annals*. The following extracts from these letters are added as still further developing them, and with them we shall close this article:

"The prevalent notion that the intense light and heat produced by galvanic action are results secondary to electricity, the presence of which is at times only indirectly discoverable, the more surprises me, since it does not in the smallest degree elucidate the primary operation, by which this principle is alleged to be evolved. According to some philosophers, the contact of the metals alone, according to others this contact accompanied by their solution, evolves electricity in quantity sufficient to extricate heat and light from a wire made the medium of transmission. They do not, however, explain why the electricity does not, according to all its known habitudes, rapidly escape through the water as fast as generated, instead of proceeding from one plate to another, in order to pass off through a second portion of the same fluid. Would it not be more philosophical to suppose that the heat and light result *directly* from the causes supposed to produce them *indirectly*, especially as we actually see *them* in a high degree of intensity, while the characteristic agency of the principle by which they are supposed to be produced, is but feebly perceived, or imperfectly demonstrated? In the case of a single galvanic pair, electricity has never been alleged discoverable, unless by the questionable assistance of condensers.

"Besides, without supposing caloric and light to circulate from the apparatus through the conjunctive wire, those who consider them as material, will find it impossible to account for the durability of the ignition. If it be supposed that these principles are extricated from the metal only by electricity passing through it, their repeated or incessant expenditure ought sooner or later to exhaust the metal, and render it incapable of further ignition."

Speaking of Dr. Silliman's account of the incompatibility of the voltaic battery with the deflagrator, Dr. Hare says:

"It cannot be doubted, notwithstanding your experiments, that there is a principle of action common to the various apparatus which you employed, and all other galvanic combinations. The effect of this principle of action, however, varies widely according to the number of the series, the size of the members severally, and the energy of the agents interposed. Towards the different extremes of these varieties are De Luc's column apparently producing pure electricity, and one large galvanic pair, or calorimotor of two surfaces, producing, in appearance, only pure caloric. At different points between these are the series of Davy and Children; the one gigantic in number, the other in size. In the deflagrator we have another variety, which, with respect to size and number, is susceptible of endless variation.

"It must be evident that no galvanic instrument where a fluid is employed could aid, or be aided by, the columns of De Luc or Zamboni. Nor could the influence of either be transmitted by the other. A calorimotor could not aid Davy's great series; nor could the latter act through a calorimotor.* Taking it for granted that there can be no oversight in your experiments, this incompatibility of exciting power must exist to a great degree under circumstances where it could hardly have been anticipated.

"Were the fluid evolved by galvanic action purely electric, the effect of batteries of different sizes, when united in one circuit, ought not to be less than would be produced if the whole of the pairs were of the smaller size. But if on the contrary we suppose the voltaic fluid compounded of caloric, light, and electricity, so obviously collateral products of galvanic action, the ordinary voltaic series employed in your experiments may owe its efficacy more to electricity, and the deflagrator more to caloric. The peculiar potency of both may be arrested when they are joined, by the incompetency of either series to convey any other compound than that which it generates. The supply of caloric from the ordinary series may be too small, that of electricity too large, and *vice versâ*. It might be expected that each would supply the deficiency of the other; but it is well known that many principles will combine only when they are nascent. The power of my large deflagrator in producing decomposition is certainly very disproportional to its power of evolving heat and light. When wires proceeding from the poles were placed very near each other under water, it was rapidly decomposed; but when severally introduced into the open ends of an inverted syphon, filled with that fluid, little action took place. Potash is deflagrated, and the rosy hue of the flame indicates a decomposition; still, however, the volatilization of the whole mass, and the intense ignition of the metallic support, prove that the calorific influence is greatly and peculiarly predominant."

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* *Unless as an inert metallic mass.*

With regard to light, Dr. H. observes, "I fear that in my essays on galvanic theory, the possible activity of light has been too much overlooked. The corpuscular changes which have been traced to the distinctive energies of this principle are so few that we have all been in the habit, erroneously perhaps, of viewing it as an inert product in those changes effected by caloric, electricity, and chemical action, which it most strikingly characterizes. Yet reflecting on the prodigious intensity in which it has been extricated by the deflagrator, it seems wrong not to suspect it of being an effective constituent of the galvanic stream. Possibly its presence in varying proportions may be one reason of the incompatibility of the voltaic current as generated under different circumstances, or by various forms of apparatus. It may also suggest, why in addition to changes in the force or nature of the sensation produced by the galvanic discharges which may be considered as dependent on electric intensity, peculiarities have been observed which are not to be thus explained. The effect on the animal frame has been alleged to be proportional to the electrical *intensity*, the effect on metals to the *quantity*; but according to the observations of Singer (which are confirmed by mine), the electrical intensity is as great with water as with acid, if not greater even than with the latter. The reverse is true of the shock. When the plates of the deflagrator are moistened and withdrawn from the acid, the shock is far less powerful; yet the electrical excitement appears stronger. Light is undeniably requisite to vegetable life; perhaps it is no less necessary in the more complicated process of animal vitality, and the electric fluid may be the mean of its distribution. The miraculous difference observed in the properties of organic products, formed of the same ponderable elements, may be due to imponderable agents conveyed and fixed in them by galvanism. Hence it may arise that the prussic acid instantaneously kills when applied to a tongue containing the same ponderable elements. When by the intense decomposition of matter light is always evolved; when an atom of tallow gives out enough of it to produce sensation in the retina of millions of living beings, why may it not, when presented in due form, influence the taste, and otherwise stimulate the nervous system? For such an office its subtilty would seem to qualify it eminently. The phenomena of the fire-fly and the glow-worm prove that it may be secreted by the process of vitality.

"The discovery of alkaline qualities, as well as acid, in organic products whose elements are otherwise found, whether separate or in combination, without any such qualities, and the opposite habitudes of acids and alkalies with the voltaic poles, and their power of combining with, and neutralizing each other, indicate that there may be something adventitious which causes alkalinity and acidity, and that this something is of an imponderable character, and dependent on galvanism.

"In the number of your journal for October last, I gave my reasons for believing in the existence of material imponderable principles producing the phenomena of heat, light, and electricity. The co-existence of these principles in the medium around us, their simultaneous or alternate agency and appearance during many of the most important processes of nature, seem to me to sanction a conjecture, that as ingredients in ponderable substances they may cause those surprisingly active and wonderfully diversified properties usually ascribed to apparently inadequate changes in the proportions of ponderable elements.

"In obedience to your request, I have thus displayed the ideas at present awakened in my mind by these obscure and interesting phenomena. I am not willing to assume any responsibility for the correctness of my conjectures. Possibly they may excite in you further and more correct speculations."

ARTICLE V.

On the Detection of very minute Quantities of Arsenic and Mercury. By James Smithson, Esq. FRS.

(To the Editor of the *Annals of Philosophy.*)

SIR,

To be able to discover exceedingly small quantities of arsenic and mercury must, on many occasions, prove conducive to the purposes of the chemist and the mineralogist, more especially now that a very diminished scale of experiment, highly to the advantage of these sciences, is becoming daily more generally adopted.

But the occasion above all others in which the power of doing this is important, are those of poisonings. In these it is often of the first moment to be able to pronounce with certainty, from portions of matter of extreme minuteness, on the existence and the nature of the poison.

Of Arsenic.

I have already communicated the method here proposed for the discovery of arsenic by employing it in the analysis of the compound sulphuret of lead and arsenic from Upper Valais, printed in the *Annals of Philosophy* for August, 1819, but not having mentioned the generality of its application, or the great accuracy of it, it seems not superfluous, from the importance of the subject, to resume it.

If arsenic, or any of its compounds, is fused with nitrate of potash, arseniate of potash is produced, of which the solution affords a brick-red precipitate with nitrate of silver.

In cases where any sensible portion of the potash of the nitre has become set free, it must be saturated with acetous acid, and the saline mixture dried and redissolved in water.

So small is the quantity of arsenic required for this mode of trial, that a drop of a solution of oxide of arsenic in water, which, at a heat of 54·5° Fahr. contains not above 1-80th of oxide of arsenic,* put to nitrate of potash in the platina spoon and fused, affords a considerable quantity of arseniate of silver. Hence when no solid particle of oxide of arsenic can be obtained, the presence of it may be established by infusing in water the matters which contain it.

The degree in which this test is sensible is readily determined.

With 5·2 grains of silver, I obtained 6·4 grains of arseniate of silver; but 0·65 grain of silver was recovered from the liquor, so that the arseniate had been furnished by 4·55 grs. of silver.

In a second trial 7·7 grains of silver, but of which only 6·8 grains precipitated, yielded 9·5 grs. of arseniate.

The mean is 140·17 from 100 of silver.

If we suppose 100 of silver to form 107·5 of oxide, we shall have

Oxide of silver	107·50
Acid of arsenic	32·67

Consequently 1 of acid of arsenic will produce 4·29 of arseniate of silver; 1 of white oxide of arsenic, 4·97; and 1 of arsenic, 6·56.

Of Mercury.

All the oxides and saline compounds of mercury laid in a drop of marine acid on gold with a bit of tin, quickly amalgamate the gold.

A particle of corrosive sublimate, or a drop of a solution of it, may be thus tried. The addition of marine acid is not required in this case.

Quantities of mercury may be rendered evident in this way which could not be so by any other means.

This method will exhibit the mercury in cinnabar. It must be previously boiled with sulphuric acid in the platina spoon to convert it into sulphate.

Cinnabar heated in solution of potash on gold amalgamates it.

A most minute quantity of metallic mercury may be discovered in a powder by placing it in nitric acid on gold, drying, and adding muriatic acid and tin.

A trial I made to discover mercury in common salt by the present method was not successful, owing, perhaps, to the smallness of the quantity which I employed.

I am, Sir, yours, &c. James Smithson.

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* Chimie de Thenard, ii. p. 167.

ARTICLE VI.

On the Apparent Right Ascension of δ Ursæ Minoris as a Verification of the Meridian Position of a Transit Instrument. By James South, FRS. &c.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Blackman-street, July* 24, 1822.

WHATEVER tends to promote accuracy in the practical part of astronomical science, is a fit object of encouragement; and if more than others there be observations in which accuracy is important, it is those of right ascension; for accordingly as these are *well* or *ill* determined, will the various observations of which they form the basis, prove *beneficial* or *injurious*. Fortunately the instrument employed for this purpose is so simple in its construction, that with a proper degree of caution on the part of its employer, it affords results little liable to error;[*] still, however, no opportunity of examining not only its adjustments, but also its position relatively to the meridian, should pass unembraced; and for the latter purpose, frequent observations, *not* of a *tottering* mark which may be *here* to-day and *there* to-morrow, but of high and low stars whose relative right ascensions are well settled, or of the superior and inferior transits of circumpolar stars, are absolutely indispensable: if the former be recurred to, no stars are so proper as those of Dr. Maskelyne's Catalogue; the mode, however, is a dependent one; the latter, therefore, where the instruments will allow of its use, being not liable to this objection, is generally preferred; and when it is remembered that the instrument which passes through the zenith and bisects one or other of these stars, at intervals of 12 hours ± the corresponding correction in right ascension, *must* move in the plane of the meridian, it is surely to be regretted that the daily corrections of the principal ones, within 15° of the pole, have not been computed. Under these circumstances the Ephemeris of the pole star, published by Mr. Baily in the Phil. Mag. of June, 1820, I have found extremely useful; and with the idea that a similar table of δ Ursæ Minoris would be little less acceptable to the practical astronomer, I avail myself of this opportunity of giving it publicity. The star is visible in the day-time, culminates about six hours after the pole star, within a few *seconds* of α Lyræ, within a few *minutes* of Sirius, and travels over the wires of the instrument nearly in half the time that the pole star does. Like the table of polaris, the accompanying is the produce of foreign industry, and the original, and, I believe, the only copy in this country, is in the possession of the Astronomical Society of London.

J. SOUTH.

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[*] As enjoying this valuable property, I by no means include those transit instruments which have stuck upon one end of their axes a circle, accurately to show (as is pretended) north polar distances. These specimens of *wisdom* are, I believe, but few in number, *and it is to be hoped* they will *remain* so.

Apparent place of δ Ursæ Minoris for every day of the year.

1822.	AR	Decl.	1822.	AR	Decl.
	18 hours.	86°		18 hours.	86°
Jan. 0	29′ 13·49″	34′ 39·35″	March 0	29′ 22·33″	34′ 23·31″
1	13·48	39·03	1	22·59	23·17
2	13·47	38·73	2	22·88	23·02
3	13·43	38·45	3	23·17	22·86
4	13·39	38·14	4	23·47	22·69
5	13·32	37·83	5	23·78	22·52
6	13·28	37·50	6	24·11	22·35
7	13·27	37·16	7	24·47	22·21
8	13 25	36·80	8	24·83	22·09
9	13·25	36·42	9	25·20	21·99
10	13·27	36·06	10	25·56	21·91
11	13·33	35·70	11	25·90	21·84
12	13·41	35·35	12	26·25	21·81
13	13·49	35·04	13	26·57	21·77
14	13·57	34·74	14	26·89	21·72
15	13·64	34·45	15	27·19	21·66
16	13·72	34·17	16	27·50	21·58
17	13·79	33·89	17	27·81	21·51
18	13·85	33·59	18	28·13	21·42
19	13·89	33·29	19	28·49	21·33
20	13·93	32·97	20	28·85	21·26
21	14·02	32·64	21	29·23	21·20
22	14·11	32·30	22	29·60	21·17
23	14·22	31·94	23	29·98	21·17
24	14·35	31·60	24	30·36	21·19
25	14·50	31·26	25	30·73	21·21
26	14·68	30·95	26	31·07	21·25
27	14·85	30·66	27	31·40	21·30
28	15·03	30·38	28	31·72	21·34
29	15·20	30·13	29	32·03	21·37
30	15·35	29·88	30	32·35	21·37
31	15·51	29·64	31	32·68	21·38
Feb. 0	15·51	29·64	April 0	32·68	21·38
1	15·66	29·38	1	33·01	21·38
2	15·81	29·11	2	33·37	21·38
3	15·96	28·82	3	33·72	21·41
4	16·11	28·51	4	34·10	21·44
5	16·29	28·22	5	34·48	21·49
6	16·50	27·91	6	34·84	21·58
7	16·72	27·62	7	35·21	21·69
8	16·96	27·34	8	35·56	21·80
9	17·21	27·07	9	35·88	21·93
10	17·46	26·83	10	36·19	22·05
11	17·73	26·62	11	36·49	22·17
12	17·99	26·42	12	36·78	22·28
13	18·22	26·22	13	37·07	22·37
14	18·45	26·01	14	37·37	22·46
15	18·67	25·81	15	37·68	22·53
16	18·90	25·60	16	38·01	22·63
17	19·13	25·36	17	38·35	22·75
18	19·36	25·11	18	38·70	22·87
19	19·61	24·87	19	39·05	23·02
20	19·89	24·62	20	39·38	23·18
21	20·19	24·40	21	39·70	23·37
22	20·50	24·20	22	40·01	23·58
23	20·82	24·01	23	40·29	23·77
24	21·13	23·84	24	40·55	23·97
25	21·45	23·69	25	40·80	24·15
26	21·76	23·57	26	41·05	24·33
27	22·05	23·44	27	41·30	24·50
28	22·33	23·31	28	41·57	24·66
			29	41·84	24·80
			30	42·14	24·9

1822.	AR	Decl.	1822.	AR	Decl.
	18 hours.	86°		18 hours.	86°
May 0	29′ 42·14″	34′ 24·95″	July 0	29′ 48·51″	34′ 42·86″
1	42·42	25·14	1	48·41	43·21
2	42·72	25·34	2	48·28	43·55
3	43·01	25·56	3	48·16	43·88
4	43·30	25·80	4	48·03	44·18
5	43·57	26·06	5	47·89	44·47
6	43·80	26·34	6	47·76	44·76
7	44·03	26·60	7	47·65	45·03
8	44·23	26·86	8	47·55	45·32
9	44·42	27·11	9	47·46	45·63
10	44·63	27·34	10	47·35	45·95
11	44·81	27·56	11	47·23	46·28
12	45·00	27·77	12	47·10	46·61
13	45·22	27·99	13	46·95	46·97
14	45·44	28·22	14	46·78	47·32
15	45·68	28·46	15	46·59	47·65
16	45·90	28·72	16	46·37	47·98
17	46·12	29·01	17	46·16	48·27
18	46·32	29·30	18	45·95	48·55
19	46·52	29·62	19	45·75	48·82
20	46·69	29·94	20	45·53	49·08
21	46·84	30·25	21	45·33	49·33
22	46·96	30·56	22	45·15	49·59
23	47·07	30·85	23	44·97	49·87
24	47·20	31·13	24	44·80	50·17
25	47·32	31·39	25	44·60	50·48
26	47·44	31·64	26	44·38	50·80
27	47·58	31·91	27	44·16	51·13
28	47·72	32·18	28	43·91	51·45
29	47·88	32·46	29	43·65	51·75
30	48·03	32·77	30	43·36	52·03
31	48·16	33·09	31	43·07	52·29
June 0	48·16	33·09	Aug. 0	43·07	52·29
1	48·28	33·43	1	42·78	52·54
2	48·39	33·78	2	42·50	52·76
3	48·47	34·13	3	42·25	52·97
4	48·53	34·47	4	41·98	53·20
5	48·57	34·79	5	41·73	53·44
6	48·60	35·10	6	41·47	53·68
7	48·64	35·40	7	41·21	53·94
8	48·68	35·69	8	40·95	54·22
9	48·73	35·96	9	40·67	54·49
10	48·78	36·25	10	40·36	54·78
11	48·85	36·54	11	40·05	55·05
12	48·92	36·86	12	39·71	55·31
13	48·98	37·19	13	39·36	55·55
14	49·04	37·54	14	39·01	55·76
15	49·06	37·90	15	38·67	55·96
16	49·06	38·27	16	38·32	56·14
17	49·04	38·63	17	38·00	56·31
18	49·01	38·98	18	37·68	56·51
19	48·98	39·32	19	37·38	56·69
20	48·93	39·63	20	37·07	56·89
21	48·87	39·93	21	36·74	57·10
22	48·82	40·23	22	36·42	57·33
23	48·80	40·51	23	36·08	57·58
24	48·78	40·81	24	35·71	57·79
25	48·75	41·11	25	35·33	58·00
26	48·73	41·43	26	34·94	58·19
27	48·69	41·77	27	34·54	58·37
28	48·66	42·13	28	34·15	58·53
29	48·61	42·50	29	33·75	58·68
30	48·51	42·86	30	33·36	58·79
			31	32·99	58·91

1822.	AR 18 hours.	Decl. 86°	1822.	AR 18 hours.	Decl. 86°
Sept. 0	29′ 32·99″	34′ 58·91″	Nov. 0	29′ 7·99″	34′ 59·99″
1	32·63	59·05	1	7·58	59·90
2	32·29	59·19	2	7·17	59·76
3	31·94	59·34	3	6·76	59·62
4	31·57	59·50	4	6·35	59·46
5	31·20	59·67	5	5·95	59·28
6	30·82	59·86	6	5·56	59·10
7	30·41	35 0·03	7	5·21	58·87
8	29·98	0·18	8	4·86	58·67
9	29·54	0·31	9	4·53	58·46
10	29·11	0·42	10	4·21	58·30
11	28·68	0·51	11	3·88	58·14
12	28·25	0·58	12	3·57	57·98
13	27·84	0·64	13	3·25	57·82
14	27·45	0·70	14	2·90	57·66
15	27·06	0·78	15	2·55	57·49
16	26·68	0·87	16	2·18	57·31
17	26·30	0·97	17	1·83	57·11
18	25·92	1·06	18	1·48	56·87
19	25·52	1·17	19	1·13	56·62
20	25·09	1·29	20	29 0·80	56·37
21	24·66	1·39	21	28 0·49	56·10
22	24·21	1·48	22	0·21	55·84
23	23·75	1·54	23	59·95	55·59
24	23·30	1·58	24	59·70	55·35
25	22·84	1·60	25	59·44	55·13
26	22·39	1·61	26	59·17	54·91
27	21·97	1·61	27	58·91	54·71
28	21·55	1·61	28	58·63	54·50
29	21·16	1·62	29	58·35	54·26
30	20·76	1·63	30	58·05	54·02
Oct. 0	20·76	1·63	Dec. 0	58·05	54·02
1	20·37	1·67	1	57·76	53·75
2	19·97	1·71	2	57·49	53·46
3	19·56	1·75	3	57·23	53·16
4	19·12	1·80	4	56·98	52·85
5	18·68	1·82	5	56·75	52·53
6	18·22	1·84	6	56·55	52·22
7	17·76	1·84	7	56·37	51·93
8	17·31	1·81	8	56·20	51·65
9	16·85	1·75	9	56·02	51·38
10	16·42	1·67	10	55·83	51·12
11	16·01	1·61	11	55·64	50·88
12	15·61	1·55	12	55·44	50·62
13	15·22	1·48	13	55·24	50·34
14	14·83	1·44	14	55·02	50·04
15	14·44	1·41	15	54·82	49·72
16	14·04	1·39	16	54·62	49·39
17	13·63	1·37	17	54·46	49·04
18	13·21	1·34	18	54·33	48·68
19	12·77	1·30	19	54·20	48·34
20	12·33	1·23	20	54·09	48·01
21	11·88	1·14	21	53·98	47·69
22	11·45	1·05	22	53·90	47·38
23	11·02	0·92	23	53·82	47·09
24	10·60	0·77	24	53·73	46·80
25	10·20	0·64	25	53·62	46·53
26	9·83	0·50	26	53·51	46·23
27	9·47	0·38	27	53·41	45·92
28	9·11	0·27	28	53·30	45·60
29	8·75	0·17		53·20	45·25
30	8·38	35 0·08	29	53·11	44·90
31	7·99	34 59·99	30	53·02	44·53
			31	52·86	44·15

Article VII.

Account of an Assemblage of Fossil Teeth and Bones, of Elephant, Rhinoceros, Hippopotamus, Bear, Tiger, and Hyæna, and 16 *other Animals; discovered in a Cave at Kirkdale, Yorkshire, in the Year* 1821: *with a Comparative View of five similar Caverns in various Parts of England, and others on the Continent.* By the Rev. William Buckland, FRS. FLS. Vice-President of the Geological Society of London, and Professor of Mineralogy and Geology in the University of Oxford, &c.*

Having been induced in December last to visit Yorkshire, for the purpose of investigating the circumstances of the cave, at Kirkdale, near Kirby Moorside, about 25 miles NNE. of the city of York, in which a discovery was made last summer of a singular collection of teeth and bones, I beg to lay before the Royal Society the result of my observations on this new and interesting case, and to point out some important general conclusions that arise from it.

The facts I have collected seem calculated to throw an important light on the state of our planet at a period antecedent to the last great convulsion that has affected its surface; and I may add, *in limine*, that they afford one of the most complete and satisfactory chains of consistent circumstantial evidence I have ever met with in the course of my geological investigations.

As I shall have frequent occasion to make use of the word *diluvium*, it may be necessary to premise that I apply it to those extensive and general deposits of superficial gravel, which appear to have been produced by the last great convulsion that has affected our planet; and that with regard to the indications afforded by geology of such a convulsion, I entirely coincide with the views of M. Cuvier, in considering them as bearing undeniable evidence of a recent and transient inundation.† On these grounds I have felt myself fully justified in applying the epithet *diluvial* to the results of this great convulsion, of *antediluvial* to the state of things immediately preceding it, and *post-diluvial* or *alluvial* to that which succeeded it, and has continued to the present time.

In detailing these observations, I propose, first, to submit a short account of the geological position and relations of the rock in which the cavern alluded to is situated; to proceed, in the next place, to a description of the cavern itself; then to enter into that which will form the most important part of this communication, a particular enumeration of the animal remains there inhumed, and the very remarkable phenomena with which they are attended; to review the general inferences to which these phenomena lead; and conclude with a brief comparative account of analogous animal deposits in other parts of this country, and the Continent.

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* From the Philosophical Transactions for 1822. Part I.

† Analogous evidences to the same point, collected in this country from the state of the gravel beds and valleys in the midland parts of England, have recently been published by myself in a paper on the Lickey Hill, in the second part of the fifth volume of the Geological Transactions, and in the Appendix to an inaugural lecture I published at Oxford, in 1820. Another paper of mine on similar evidences afforded by the valleys that intersect the coast of West Dorset and East Devonshire, will be published in the first part of the sixth volume of the Geological Transactions.

Kirkdale is situated about 25 miles NNE. of the city of York, between Helmsley and Kirby Moorside, near the point at which the east base of the Hambleton hills, looking towards Scarborough, subsides into the vale of Pickering, and on the S. extremity of the mountainous district known by the name of the Eastern and the Cleveland Moorlands.

The substratum of this valley of Pickering is a mass of stratified blue clay, identical with that which at Oxford and Weymouth reposes on a similar limestone to that of Kirkdale, and containing subordinately beds of inflammable bituminous shale, like that of Kimeridge, in Dorsetshire. Its south boundary is formed by the Howardian hills, and by the elevated escarpment of the chalk that terminates the Wolds towards Scarborough. Its north frontier is composed of a belt of limestone, extending eastward 30 miles from the Hambleton hills, near Helmsley, to the sea at Scarborough, and varying in breadth from four to seven miles; this limestone is intersected by a succession of deep and parallel valleys (here called dales) through which the following rivers from the moorlands pass down southwards to the vale of Pickering, viz. the Rye, the Rical, the Hodge Beck, the Dove, the Seven Beck, and the Costa; their united streams fall into the Derwent above New Malton, and their only outlet is by a deep gorge, extending from near this town down to Kirkham, the stoppage of which would at once convert the whole vale of Pickering into an immense inland lake; and before the excavation of which, it is probable, that such a lake existed, having its north border nearly along the edge of the belt of limestone just described, and at no great distance from the mouth of the cave at Kirkdale.

The position of the cave is at the south and lower extremity of one of these dales (that of the Rical Beck), at the point where it falls into the vale of Pickering, at the distance of about a furlong from the church of Kirkdale, and near the brow of the left flank of the valley, close to the road. This flank slopes towards the river at an angle of 25°, and the height of the brow of the slope above the water may be about 120 feet. (See Pl. XIV. fig. 1.)

The rock perforated by the cave is referable to that portion of the oolite formation which, in the south of England, is known by the name of the Oxford oolite and coral rag: its organic remains are identical with those of the Heddington quarries near Oxford, but its substance is harder and more compact, and more interspersed with siliceous matter, forming irregular concretions, beds, and nodules of chert in the limestone, and sometimes entirely penetrating its coralline remains. The most compact beds of this limestone resemble the younger alpine limestone of Meillierie and Aigle, in Switzerland, and they alternate with, and pass gradually into, those of a coarser oolitic texture; and both varieties are stratified in beds from one to four feet thick. The cave is situated in one of the compact beds which lies between two others of the coarser oolitic variety; the latter vary in colour from light-yellow to blue; the compact beds are of a dark grey passing to black, are extremely fetid, and full of corals and spines of the echinus cidaris. The compact portions of this oolite partake of the property common to compact limestones of all ages and formations, of being perforated by irregular holes and caverns intersecting them in all directions; the cause of these cavities has never been satisfactorily ascertained: into this question (which is one of considerable difficulty in geology) it is foreign to my present purpose to inquire any further than to state that they were neither produced, enlarged, or diminished by the presence of the animals whose bones we now find in them.

The abundance of such caverns in the limestone of the vicinity of Kirkdale is evident from the fact of the engulphment of several of the rivers above enumerated in the course of their passage across it from the eastern moorlands to the vale of Pickering; and it is important to observe that the elevation of the Kirkdale cave, above the bed of the Hodge Beck, exceeding 100 feet, excludes the possibility of our attributing the muddy sediment we shall find it to contain, to any land flood or extraordinary rise of the waters of that or any other now existing river.

It was not till the summer of 1821 that the existence of any animal remains, or of the cavern containing them, had been suspected. At this time, in continuing the operations of a large quarry along the brow of the slope just mentioned (Pl. XIV. fig. 1), the workmen accidentally intersected the mouth of a long hole or cavern, closed externally with rubbish, and overgrown with grass and bushes. As this rubbish was removed before any competent person had examined it, it is not certain whether it was composed of diluvial gravel and rolled pebbles, or was simply the debris that had fallen from the softer portions of the strata that lay above it; the workmen, however, who removed it, and some gentlemen who saw it, assured me, that it was composed of gravel and sand. In the interior of the cave there was not a single rolled pebble, nor one bone, or fragment of bone, that bears the slightest mark of having been rolled by the action of water. A few bits of limestone and roundish concretions of chert that had fallen from the roof and sides, were the only rocky fragments that occurred, with the exception of stalactite.

About 30 feet of the outer extremity of the cave have now been removed, and the present entrance is a hole in the perpendicular face of the quarry less than five feet square, which it is only possible for a man to enter on his hands and knees, and which expands and contracts itself irregularly from two to seven feet in breadth and height, diminishing, however, as it proceeds into the interior of the hill. The cave is about 15 or 20 feet below the incumbent field, the surface of which is nearly level, and parallel to the stratification of the limestone, and to the bottom of the cave. Its main direction is ESE. but deviating from a straight line by several zigzags to the right and left (Pl. XIV. fig. 3); its greatest length is from 150 to 200 feet. In its interior it divides into several smaller passages, the extent of which has not been ascertained. In its course it is intersected by some vertical fissures, one of which is curvilinear, and again returns to the cave; another has never been traced to its termination; while the outer extremity of a third is probably seen in a crevice or fissure that appears on the face of the quarry, and which closes upwards before it leaves the body of the limestone. By removing the sediment and stalactite that now obstruct the smaller passages, a further advance in them may be rendered practicable. The half corroded fragments of corals, of spines of echini and other organic remains, and the curious ledges of limestone and nodules of chert that project along the sides and roof of the cave, together with the small grooves and pits that cover great part of its interior, show that there was a time when its dimensions were less than at present; though they fail to prove by what cause it was originally produced. There are but two or three places in which it is possible to stand upright, and these are where the cavern is intersected by the fissures; the latter of which continue open upwards to the height only of a few feet, when they gradually close, and terminate in the body of the limestone: they are thickly lined with stalactite, and are attended by no fault or slip of either of their sides. Both the roof and floor, for many yards from the entrance, are composed of horizontal strata of limestone, uninterrupted by the slightest appearance of fissure, fracture, or stony rubbish of any kind; but further in, the roof and sides become irregularly arched, presenting a very rugged and grotesque appearance, and being studded with pendent and roundish masses of chert and stalactite; the bottom of the cavern is visible only near the entrance; and its irregularities, though apparently not great, have been filled up throughout to a nearly level surface, by the introduction of a bed of mud or sediment, the history of which, and also of the stalactite, I shall presently describe. (See Plate XIV. fig. 2.)

The fact already mentioned of the engulphment of the Rical Beck, and other adjacent rivers, as they cross the limestone, showing it to abound with many similar cavities to those at Kirkdale, renders it likely that hereafter similar deposits of bones may be discovered in this same neighbourhood; but accident alone can lead to such discovery, as it is probable the mouths of these caverns are buried under diluvian sand and gravel, or post-diluvian detritus; so that nothing but their casual intersection by some artificial operations will lead to the knowledge of their existence; and in this circumstance we also see a reason why so few caverns of this kind have hitherto been discovered, although it is probable that many such may exist.

In all these cases, the bones found in caverns are never mineralised, but simply in the state of grave bones, or incrusted by stalactite; and have no further connection with the rocks themselves than that arising from the accident of having been lodged in their cavities at periods long subsequent to the formation and consolidation of the strata in which these cavities occur.

On entering the cave at Kirkdale (see Pl. XIV. fig. 2), the first thing we observe is a sediment of mud, covering entirely its whole bottom to the average depth of about a foot, and entirely covering and concealing the subjacent rock, or actual floor of the cavern. Not a particle of mud is found attached either to the sides or roof; nor is there a trace of it adhering to the sides or upper portions of the transverse fissures, or any thing to suggest the idea that it entered through them. The surface of this sediment, when the cave was first entered, was nearly smooth and level, except in those parts where its regularity had been broken by the accumulation of stalagmite above it, or ruffled by the dripping of water: its substance is argillaceous and slightly micaceous loam, composed of such minute particles as would easily be suspended in muddy water, and mixed with much calcareous matter, that seems to have been derived in part from the dripping of the roof, and in part from comminuted bones.

Above this mud, on advancing some way into the cave, the roof and sides are seen to be partially studded and cased over with a coating of stalactite, which is most abundant in those parts where the transverse fissures occur, but in small quantity where the rock is compact and devoid of fissures. Thus far it resembles the stalactite of ordinary caverns; but on tracing it downwards to the surface of the mud, it was there found to turn off at right angles from the sides of the cave, and form above the mud a plate or crust, shooting across like ice on the surface of water, or cream on a pan of milk. (See Pl. XIV. fig. 2.) The thickness and quantity of this crust varied with that found on the roof and sides, being most abundant, and covering the mud entirely where there was much stalactite on the sides, and more scanty in those *places where* the roof presented but little: in many parts it was totally wanting both on the roof and surface of the mud and subjacent floor. Great portion of this crust had been destroyed in digging up the mud to extract the bones; it still remained, however, projecting partially in some few places along the sides; and in one or two, where it was very thick, it formed, when I visited the cave, a continuous bridge over the mud entirely across from one side to the other. In the outer portion of the cave, there was a mass of this kind which had been accumulated so high as to obstruct the passage, so that a man could not enter till it had been dug away.

These horizontal incrustations have been formed by the water which, trickling down the sides, was forced to ooze off laterally, as soon as it came into contact with the mud; in other parts, where it fell in drops from the roof, stalagmitic accumulations have been raised on its surface, some of which are very large, but more commonly they are of the size and shape of a cow's pap, a name which the workmen have applied to them. There is no alternation of mud with any repeated beds of stalactite, but simply a partial deposit of the latter on the floor beneath it; and it was chiefly in the lower part of the sediment above described, and in the stalagmitic matter beneath it, that the animal remains were found: its substance contains no black earth or admixture of animal matter, except an infinity of extremely minute particles of undecomposed bone. In the whole extent of the cave, only a very few large bones have been discovered that are tolerably perfect; most of them are broken into small angular fragments and chips, the greater part of which lay separately in the mud, while others were wholly or partially invested with stalactite; and some of the latter united with masses of still smaller fragments, and cemented by the stalactite, so as to form an osseous breccia, of which I have specimens.

The effect of this mud in preserving the bones from decomposition has been very remarkable; some that had lain a long time before its introduction were in various stages of decomposition; but even in these, the further progress of decay appears to have been arrested by it; and in the greater number, little or no destruction of their form, and scarcely any of their substance, has taken place. I have found on immersing fragments of these bones in an acid till the phosphate and carbonate of lime were removed, that nearly the whole of their original gelatine has been preserved. Analogous cases of the preservative powers of diluvial mud occur on the coast of Essex, near Walton, and at Lawford, near Rugby, in Warwickshire. Here the bones of the same species of elephant, rhinoceros, and other diluvial animals, occur in a state of freshness and freedom from decay, nearly equal to those in the cave at Kirkdale, and this from the same cause, viz. their having been protected from the access of atmospheric air, or the percolation of water, by the argillaceous matrix in which they have been imbedded; while similar bones that have lain the same length of time in diluvial sand, or gravel, and been subject to the constant percolation of water, have lost their compactness and strength and great part of their gelatine, and are often ready to fall to pieces on the slightest touch; and this where beds of clay and gravel occur alternating in the same quarry, as at Lawford.

The workmen on first discovering the bones at Kirkdale, supposed them to have belonged to cattle that died by a murrain in this district a few years ago, and they were for some time neglected, and thrown on the roads with the common limestone; they were at length noticed by Mr. Harrison, a medical gentleman of Kirby Moorside, and have since been collected and dispersed among so many individuals, that it is probable nearly all the specimens will in a few years be lost, with the exception of such as may be deposited in public collections. By the kindness and liberality of the Bishop of Oxford (to whom I am also indebted for my first information of the discovery of this cave), and of C. Duncombe, Esq. and Lady Charlotte Duncombe, of Duncombe Park, a nearly complete series of the teeth of all these animals has been presented to the Museum at Oxford; while a still better collection both of teeth and bones is in the possession of J. Gibson, Esq. of Stratford, in Essex, to whose exertions we owe the preservation of many valuable specimens, and who is about to present a series of them to our public collections in London. W. Salmond, Esq. also, since I visited Kirkdale in December last, has been engaged with much zeal and activity in measuring and exploring new branches of the cave, and making large collections of the teeth and bones, from which I understand he also intends to enrich our public cabinets in the metropolis. I am indebted to him for the annexed ground plan of the cave, and its ramifications (Pl. XIV. fig. 3).* Drawings by Mr. Clift, of some of the most perfect of Mr. Gibson's specimens, have been sent to M. Cuvier, for the new edition of his work on fossil animals; copies of these have been made for me by Miss Morland, and appear in the annexed plates, with many other drawings, for which I am indebted to the pencil of Miss Duncombe; and the Rev. George Young, and Mr. Bird, of Whitby, in their History of the Geology of the coast of Yorkshire, have given engravings of some teeth that remain in their possession.

It appears that the teeth and bones which have as yet been discovered in the cave at Kirkdale, are referable to the following 22 species of animals.

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* Plan of the cave drawn and measured by W. Salmond, Esq. The figures within the lines express the width of the cave in feet and inches, those outside its height. Both these have been enlarged by removing stones to obtain a passage.

A. Original slope of the hill.
B. Rubbish filling the mouth of the cave.
C. Original entrance of solid rock.
D. Portion of cave destroyed by quarrying.
E. Present entrance of cave.

7 Carnivora.—Hyæna, tiger, bear, wolf, fox, weasel, and an unknown animal of the size of a wolf.

4 Pachydermata.—Elephant, rhinoceros, hippopotamus, and horse.

4 Ruminantia.—Ox, and three species of deer.

3 Rodentia.—Rabbit, water-rat, and mouse.

4 Birds.—Raven, pigeon, lark, and a small species of duck, resembling the anas sponsor, or summer duck.

The bottom of the cave, on first removing the mud, was found to be strewed all over like a dog kennel, from one end to the other, with hundreds of teeth and bones, or rather broken and splintered fragments of bones, of all the animals above enumerated; they were found in greatest quantity near its mouth, simply because its area in this part was most capacious; those of the larger animals, elephant, rhinoceros, &c. were found co-extensively with all the rest, even in the inmost and smallest recesses (see Pl. XIV. fig. 3). Scarcely a single bone has escaped fracture, with the exception of the astragalus, and other hard and solid bones of the tarsus and carpus joints, and of the toes. On some of the bones marks may be traced, which, on applying one to the other, appear exactly to fit the form of the canine teeth of the hyæna that occur in the cave. The hyænas' bones have been broken, and apparently gnawed equally with those of the other animals. Heaps of small splinters, and highly comminuted, yet angular fragments of bone, mixed with teeth of all the varieties of animals above enumerated, lay in the bottom of the den, occasionally adhering together by stalactite, and forming, as has been before mentioned, an osseous breccia. Many insulated fragments also are wholly or partially enveloped with stalactite, both externally and internally. Not one skull is to be found entire; and it is so rare to find a large bone of any kind that has not been more or less broken, that there is no hope of obtaining materials for the construction of any thing like a skeleton. The jaw bones also, even of the hyænas, are broken like the rest; and in the case of all the animals, the number of teeth and of solid bones of the tarsus and carpus, is more than twenty times as great as could have been supplied by the individuals whose other bones we find mixed with them.

Fragments of jaw bones are by no means common: the greatest number I saw belong to the deer, hyæna, and water-rat, and retain their teeth; in all the jaws both teeth and bone are in an equal high state of preservation, and show that their fracture has been the effect of violence, and not of natural decay. I have seen but 10 fragments of deers' jaws, and about 40 of hyænas' (see Pl. XV. fig. 2, 3), and as many of rats. The ordinary fate of the jaw bones, as of all the rest, appears to have *been to be broken* to pieces.

The greatest number of teeth are those of hyænas, and the uminantia. Mr. Gibson alone collected more than 300 canine eeth of the hyæna, which at the least must have belonged to 75 ndividuals, and they are in the same proportion in other collections. The only remains that have been found of the tiger species are two large canine teeth, each four inches in length, and one molar tooth, exceeding in size that of the largest lion or Bengal tiger. There is one tusk only of a bear which exactly resembles those of the extinct ursus spelæus of the caves of Germany, the size of which M. Cuvier says must have equalled that of a large horse. Of the wolf and fox there are many teeth, and others belonging to an animal which I cannot ascertain: it seems to have been nearly allied to the wolf, but the teeth are much thinner, and less strong. A few jaws and teeth have also been found belonging to the weasel. Teeth of the larger pachydermatous animals are not abundant. I have information of about 10 elephants' teeth, but of no tusk; and as very few of these teeth exceed three inches in their longest diameter, they must have belonged to very young animals. I have seen but six molar teeth of the hippopotamus, and a few fragments of its canine and incisor teeth; some of which latter are in the possession of Mr. Thorpe, of York. Teeth of the rhinoceros are not so rare. I have seen 40 or 50, and some of them extremely large ones, and apparently from aged animals. I have heard of only two or three teeth belonging to the horse. Of the teeth of deer there are at least three species, the smallest being very nearly of the size and form of those of a fallow deer, the largest agreeing in size, but differing in form from those of the modern elk; and a third being of an intermediate size, and approaching that of a large stag or red deer. I have not ascertained how many species there are of ox, but apparently there are at least two. But the teeth which occur perhaps in greatest abundance, are those of the water-rat; for in almost every specimen I have collected or seen of the osseous breccia, there are teeth or broken fragments of the bones of this little animal mixed with, and adhering to the fragments of all the larger bones. These rats may be supposed to have abounded on the edge of the lake, which I have shown probably to have existed at that time in this neighbourhood: there are also a few teeth and bones of rabbits and mice.

Besides the teeth and bones already described, the cave contained also remains of horns of at least two species of deer. One of these resembles the horn of the common stag or red deer, the circumference of the base measuring 9¾ inches, which is precisely the size of our largest stag. A second measures 7¾ inches at the same part, and both have two antlers, that rise very near the base. In a smaller species, the lowest antler is 3½ inches above the base, the circumference of which is 8 inches. No horns are found entire, but fragments only, and these apparently gnawed to pieces like the bones: their lower extremity nearest the head is that which has generally escaped destruction: and it is a curious fact, that this portion of all the horns I have seen from the cave shows, by the rounded state of the base, that they had fallen off by absorption or necrosis, and been shed from the head on which they grew, and not broken off by violence.

It must already appear probable, from the facts above described, particularly from the comminuted state and apparently gnawed condition of the bones, that the cave at Kirkdale was, during a long succession of years, inhabited as a den by hyænas, and that they dragged into its recesses the other animal bodies whose remains are found mixed indiscriminately with their own; and this conjecture is rendered almost certain by the discovery I made, of many small balls of the solid calcareous excrement of an animal that had fed on bones, resembling the substance known in the old Materia Medica by the name of album græcum: its external form is that of a sphere, irregularly compressed, as in the fæces of sheep, and varying from half an inch to an inch in diameter; its colour is yellowish-white, its fracture is usually earthy and compact, resembling steatite, and sometimes granular; when compact, it is interspersed with minute cellular cavities: it was at first sight recognised by the keeper of the Menagerie at Exeter Change, as resembling both in form and appearance, the fæces of the spotted or Cape Hyæna, which he stated to be greedy of bones, beyond all other beasts under his care. This information I owe to Dr. Wollaston, who has also made an analysis of the substance under discussion, and finds it to be composed of the ingredients that might be expected in fæcal matter derived from bones, viz. phosphate of lime, carbonate of lime, and a very small proportion of the triple phosphate of ammonia and magnesia; it retains no animal matter, and its originally earthy nature and affinity to bone will account for its perfect state of preservation.

I do not know what more conclusive evidence than this can be added to the facts already enumerated, to show that the hyænas inhabited this cave, and were the agents by which the teeth and bones of the other animals were there collected; it may be useful, therefore, to consider, in this part of our inquiry, what are the habits of modern hyænas, and how far they illustrate the case before us.

The modern hyæna (of which there are only three known species, all of them smaller and different from the fossil one) is an inhabitant exclusively of hot climates; the most savage, or striped species, abounds in Abyssinia, Nubia, and the adjacent parts of Africa and Asia. The less ferocious, or spotted one, inhabits the Cape of Good Hope, and lives principally on carrion. In bony structure the latter approaches more nearly than the former to the fossil species: to these M. Cuvier adds a third, the red hyæna, which is very rare.

The structure of these animals places them in an intermediate class between the cat and dog tribes; not feeding, like the former, almost exclusively on living prey, but like the latter, being greedy also of putrid flesh and bones:* their love of putrid flesh induces them to follow armies, and dig up human bodies from the grave. They inhabit holes which they dig in the earth, and chasms of rocks; are fierce, and of obstinate courage, attacking stronger quadrupeds than themselves, and even repelling lions. Their habit of digging human bodies from the grave, and dragging them to their den, and of accumulating around it the bones of all kinds of animals, is thus described by Busbequius, where he is speaking of the Turkish mode of burial in Anatolia, and their custom of laying large stones upon their graves to protect them from the hyænas. "Hyæna regionibus iis satis frequens; sepulchra suffodit, extrahitque cadavera, portatque ad suam speluncam; juxta quam videre est ingentem cumulum ossium humanorum 'veterinariorum,† et reliquorum omne genus animalium." (Busbeq. Epist. 1. Leg. Turc.) Brown, also, in his Travels to Darfur, describes the hyænas' manner of taking off their prey in the following words:—"They come in herds of six, eight, and often more, into the villages at night, and carry off with them whatever they are able to master; they will kill dogs and asses even within the enclosure of houses, and fail not to assemble wherever a dead camel or other animal is thrown, which, acting in concert, they sometimes drag to a prodigious distance." Sparman and Pennant mention that a single hyæna has been known to carry off a living man or woman in the vicinity of the Cape.

The strength of the hyæna's jaw is such, that in attacking a dog, he begins by biting off his leg at a single snap. The capacity of his teeth for such an operation is sufficiently obvious from simple inspection, and had long ago attracted the attention of the early naturalists; and, consistent with this strength of teeth and jaw, is the state of the muscles of his neck, being so full and strong, that in early times this animal was fabled to have but one cervical vertebra. They live by day in dens, and seek their prey by night, having large prominent eyes, adapted, like those of the rat and mouse, for seeing in the dark. To animals of such a class, our cave at Kirkdale would afford a most convenient habitation, and the circumstances we find developed in it are entirely consistent with the habits above enumerated.

It appears from the researches of M. Cuvier, that the fossil hyæna was nearly one-third larger than the largest of the modern species; that is, the striped or Abyssinian; but in the structure of its teeth, more nearly resembled that of the Cape animal. (See Plate XV. fig. 1, 2, 3.) Its muzzle also was shorter and stronger than in either of them, and consequently its bite more powerful. The length of the largest modern hyæna noticed is five feet nine inches.

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* It is quite impossible to mistake the jaw of any species of hyæna for that of the wolf or tiger kind; the latter having three molar teeth only in the lower jaw, and the former seven; while all the hyæna tribe have four. (See Plate XV. fig. 1, 2, 3.)

† Veterinam bestiam jumentum Cato appellavit a vehendo: (quasi veheterinus vel veterinus.) Pomp. Fest.

The fossil species has been found on the Continent in situations of two kinds, both of them consistent with the circumstances under which it occurs in Yorkshire, and, on comparing the jaws and teeth of the latter with those of the former engraved in M. Cuvier's Recherches sur les Ossements Fossiles, I find them to be absolutely identical. The two situations are caverns and diluvian gravel.

1. In Franconia, a few bones of hyæna were found mixed with those of an enormous number of bears, in the cave of Gailenreuth.

2. At Muggendorf, in a similar cave.

3. At Bauman, in ditto.

4. At Fouvent, near Gray, in the department of Doubes, bones of hyæna were found mixed with those of the elephant and horse in a fissure of limestone rock, which, like that at Kirkdale, was discovered by the accidental digging away of the rock in a garden.

5. At Canstadt, in the valley of the Necker, A. D. 1700, hyænas' bones were found mixed with those of the elephant, rhinoceros, and horse, and with rolled pebbles, in a mass of yellowish clay.

6. Between Hahldorf and Reiterbuck, on the surface of the hills that bound the valley of Eichstadt, in Bavaria. These were buried in a bed of sand.

The four first of these cases appear to have been dens, like the cave at Kirkdale; the two latter are deposits of diluvian detritus, like the surface gravel beds of England, in which similar remains of all the other animals have been found, excepting hyænas.

It has been observed when speaking of the den, that the bones of the hyænas are as much broken to pieces as those of the animals that formed their prey; and hence we must infer that the carcases even of the hyænas themselves were eaten up by their survivors. Whether it be the habit of modern hyænas to devour those of their own species that die in the course of nature; or under the pressure of extreme hunger to kill and eat the weaker of them, is a point on which it is not easy to obtain positive evidence. Mr. Brown, however, asserts, in his journey to Darfur, "that it is related of the hyænas, that upon one of them being wounded, his companions instantly tear him to pieces and devour him." It seems, therefore, in the highest degree probable, that the mangled relics of hyænas that lie indiscriminately scattered and equally broken with the bones of other animals in the cave of Kirkdale, were reduced to this state by the agency of the surviving individuals of their own species.

A large proportion of the hyænas' teeth bear marks of extreme old age, some being abraded to the very sockets, and the majority having lost the upper portion of their coronary part, and having fangs extremely large: these probably died in the den from mere old age: and if we compare the lacerated condition of the bones that accompany them, with the state of the teeth thus worn down to the very stumps, notwithstanding their prodigious strength, we find in the latter the obvious instruments by which the former were thus comminuted. A great number of other teeth appear to have belonged to young hyænas, for the fangs are not developed, and the points and edges of the crown are not the least worn down. I have a fragment of the jaw of an hyæna which died so young, that the second set of its teeth had not been protruded, but were in the act of forming within the jaw. Others are in various stages of advancement towards maturity; and the proportion of these is too great for us to attribute them to animals that may have died in early life from accident or disease. It seems more probable, and the idea is confirmed by the above statement of Mr. Brown, and by the fact of the hyænas' bones in the den being gnawed and broken to pieces equally with the rest, that they were occasionally killed and devoured by the stronger individuals of their own species.

But besides the evidence their teeth afford to show that the animals died at various periods of life, they present other appearances (and so likewise do the bones), of having passed through different stages and gradations of decay, arising from the different length of time they had lain exposed in the bottom of the den, before the muddy sediment entered, which, since its introduction, has preserved them from further decomposition. This observation applies equally to all the animals. I have portions of bone and teeth that are so much decomposed as to be ready to fall to pieces by the slightest touch; these had probably lain a long time unprotected in the bottom of the den; others still older may have entirely perished; but the majority both of teeth and fragments of bone are in a state of the highest preservation; and many thousands have been collected and carried away since the cave was discovered. In all cases the degree of decay is equal in the teeth and jaw bones, or fragments of jaws, to which they are attached.

(*To be continued.*)

ARTICLE VIII.

On Diaspore. By J. G. Children, Esq. FRS. &c.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *British Museum, July* 24, 1822.

In the third volume (New Series) of the *Annals of Philosophy*, p. 433, Mr. G. B. Sowerby has published his discovery of a new variety of Diaspore, together with some experiments which I made, at his desire, on a portion of it by the blowpipe, and in the following volume, p. 17, your brother, Mr. W. Phillips, with his usual ability, has described the crystalline form of a similar substance in the possession of Mr. S. L. Kent, a fragment of which I have also examined, and am satisfied of its identity with the former.

I have subsequently submitted the mineral to a further analysis, and, I believe, the results as stated below, are a pretty near approximation to the truth, though the quantity on which I operated was necessarily small, notwithstanding Mr. Sowerby's liberality, who would willingly have furnished me with larger portions of this very rare substance, had I thought it right to consent to the sacrifice.

The quantity of water was ascertained by heating the mineral to redness, in which operation pure water only was given off. The heated portion was fused with about eight times its weight of borax, the mass dissolved in diluted muriatic acid, and the whole precipitated by carbonate of potassa. The precipitate, well washed, was collected from the filter while in a moist state, and treated with a solution of pure potassa, which left the oxide of iron untouched; and, lastly, the alumina was separated from the alkali by muriate of ammonia.

The use of borax for the fusion of aluminous stones was, I believe, first recommended by Mr. Chenevix, and is the best flux for such minerals that I am acquainted with; but in the subsequent precipitation of the alumina from its solution in the muriatic acid, by carbonate of potassa, it is necessary to concentrate the solution by evaporation (for the glass requires a rather large quantity of fluid to dissolve it), or a considerable proportion will escape the action of the precipitant, even though boiled. I was nearly led into a serious error by not being aware of this circumstance.

It is stated, in Mr. Sowerby's communication, that the test of boracic acid and iron before the blowpipe gave no trace of the presence of a phosphate in the mineral; and I equally failed in detecting any, by treating a small portion with soda and silica, in the manner used by Berzelius in his excellent analysis of Wavellite. I also made a separate experiment to ascertain if the diaspore contain an alkali, by fusing it with nitrate of baryta, but of this also I could discover no trace. The result of my analysis conducted as above gave

Alumina	76·06
Protoxide of iron	7·78
Water	14·70
Loss	1·46
	100·00

Perhaps the true proportions may be:

		Equivalents.	
Alumina	76·923 =	20 =	360
Protoxide of iron	7·692 =	1 =	36
Water	15·385 =	8 =	72
	100·000		468

Lelievre's diaspore is accompanied by a dark coloured substance, which has been supposed to be a mere variety of the lighter, but by the following experiment, on a minute portion furnished by Mr. Sowerby, before the blowpipe, that does not appear to be the case.

In the matrass, if freed from the true diaspore, the assay does not decrepitate. Its dark brown (almost black) colour becomes rather lighter, and it gives off a large quantity of water. Alone in the forceps it does not fuse. The heated fragment does not brown moistened turmeric paper.

With soda, on platina wire, in the oxidating flame, it gives a light opaque dirty brown globule. In the reducing flame, the colour is darker, and somewhat inclining to bottle-green.

On platina foil, with soda and nitre, it gives no trace of manganese.

With borax, on platina wire, in the oxidating flame, fuses slowly into a perfectly transparent glass, deep orange-red while hot, fine yellow when cold, and which does not become opaque by flaming. In the reducing flame, the colour of the globule changes to bottle-green.

With salt of phosphorus, on the platina wire, in the oxidating flame, it dissolves slowly, but perfectly, into a diaphanous glass of a fine deep orange colour while hot, which, on cooling, becomes lemon-yellow, and when quite cold is colourless. In the reducing flame the assay presents the same phenomena.

A portion of the pulverised assay treated with a drop of nitrate of cobalt on charcoal, in the usual manner, gave a black mass.

Vauquelin's analysis of Lelievre's diaspore gave

Alumina	80
Iron	3
Water	17

quantities that are not reconcileable to equivalent proportions.

Yours truly,
John George Children.

Article IX.

Analyses of Books.

Memoirs of the Astronomical Society of London. Vol. I. London. 1822.

Those anticipations in which we ventured to indulge, when announcing the formation of the Astronomical Society, the contents of the present volume have fully justified. The list of its members comprehending names unquestionably the most distinguished among the scientific, and the well-known zeal of many in the practice of astronomy, gave assurance that numerous valuable communications would soon be presented to it. From the distinction which has been acquired by the artists of this country in the construction of astronomical instruments, it might naturally be expected that practical contributions of this description would frequently appear; and accordingly among these memoirs, the first and second convey the results of the ingenious labours of Troughton and Dollond; the former giving "an account of the repeating circle, and of the altitude and azimuth instrument; describing their different constructions, the manner of performing their principal adjustments, and how to make observations with them, together with a comparison of their respective advantages;" and the latter offering "the description of a repeating instrument upon a new construction."

That the repeating circle, introduced for the correction of imperfections in the art of dividing, should not be approved by Mr. Troughton, who has so greatly advanced that art, and still so actively labours to perfect it, cannot occasion any surprise. Even its form and general appearance are objected to by him, for it is stated to be "of all the instruments subservient to geodesy and astronomy, the most uncouth and unsightly." He adds, "the whole of the effective parts are placed on one side of its single supporting pillar, and on the other a weight almost equal to the instrument, is placed for the purpose of keeping it in equilibrio. But ugliness is not the worst thing that attends this unavoidable combination; for it renders the instrument *top-heavy, tottering*, and weak. In these respects, the azimuth circle is very much superior. The whole of its fabric is regular and self-balanced; the upper circle being supported like a transit upon two columns is thus rendered firm and steady. Respecting sightliness, I think the man of taste would, in the different forms it has appeared under, pronounce it agreeable, I dare not say beautiful; and here I may be allowed to remark, that the art of instrument making, as a matter of taste, is far behind many others. In this country indeed at the beginning of the art, instruments were adorned with the flourish of the engraver, chaser, and carver (now long out of fashion); but these are not the beauties which I mean; those of uniformity of figure and just proportions are alone what I have in view; and I cannot for a moment think that these are at all inconsistent either with strength or accuracy. Through the whole of this paper, every reader will have seen that I am an advocate for the altitude and azimuth instrument, and I have made no endeavour to conceal it; yet if I have said more for it than it deserves, or given to the repeating circle less than its due, it is a thing I am quite unconscious of." How different is the opinion of the celebrated Biot respecting this degraded instrument, the following quotation from the Traité Elementaire d'Astronomie Physique will show: "L'erreur des divisions est donc comme nulle dans les observations faites au cercle. Il est impossible qu'elle soit aussi rigoureusement detruite dans les plus grands instrumens s'ils ne sont pas répétiteurs. *Jamais l'addresse de l'artiste ne peut égaler un procédé mathematique.*" (Tome 1, chap. xx. p. 278, Edit. Seconde.) Should Mr. Troughton candidly and attentively peruse the elaborate disquisition, entitled, "Description et Usages du Cercle repetiteur," he may be induced to discard the predictions which he has advanced in the concluding paragraphs of his essay. "As it was the rudeness and inaccuracy of dividing which brought this instrument into existence, we should think that as the art becomes cultivated, it will fall into disuse. The art in this country is sufficiently advanced to set repeating instruments aside; and if I am rightly informed, several foreign artists are at this time pursuing the course of its improvement, in which they had for many years been impeded by circumstances which science could not controul. It is, therefore, my opinion, that as the division of instruments becomes generally improved, so will the repeating circle hasten to its dissolution; and, perhaps, on account of the great services which in its time it has rendered to astronomy and geodesy, some future age may be induced to chaunt its *requiem.*"

The repeating instrument, of which the construction is described in the second memoir, was finished in Jan. 1819, and is stated by Mr. Dollond to be applicable to all the uses where vertical and horizontal angles are required to be taken. It may be sufficient for our present purpose to point out the novelties by which it is distinguished. The first novelty is the transverse or transit axis. By this construction, the telescope is rendered independent of the plane of the circle, and by the length of the axis is impelled to move in a truly vertical plane; and by reversing the axis, the line of collimation may be perfectly verified. This, therefore, renders it also a good instrument for observations in right ascension. The second novelty is the application of the two small levels or finders, which afford a very great convenience when repeating zenith distances; as by this application the telescope can be readily placed to those distances each time the instrument is reversed without the aid of a divided circle. There is also a novelty applied to the lantern which will be found extremely convenient. It consists of two plates of brass, having a square hole in each; these plates are moved in contrary directions by rack and pinion; and by this contrivance the observer is enabled to regulate the light in any proportion that may be required. There is also an entirely new application, which will be extremely advantageous when taking horizontal angles. This is the level which is applied to the principal horizontal circle, and which in every respect answers the purpose of a second telescope, while it is much more convenient, as the observer can instantly perceive the least possible motion of the circle without the necessity of changing his position; and if it should be required to take horizontal angles at night, the advantage will be very considerable. There is, lastly, a new appendage which will be found very useful when repeating the vertical angles. It consists of two arms fitted to the lower end of the centre that belongs to the horizontal circle, and has a motion sufficiently tight to keep it at the place to which it is set. When the telescope is presented to the object for observation, one of these arms is brought to coincide with a projecting piece in the triangular frame, and when the instrument is turned half round by bringing the other or opposite arm to coincide with the same projecting piece, the object will be again in the field of view of the telescope.

In the third memoir, Mr. Francis Baily details "A Method of fixing a Transit Instrument exactly in the Meridian." This zealous and distinguished mathematician and astronomer, recommends, that when the transit instrument is placed nearly in the plane of the meridian, its accurate adjustment should be completed by observing the culmination of any two stars differing from each other considerably in declination. By this method, the necessity of having a building constructed, so as to command an uninterrupted view of the meridian from the northern to the southern horizon is avoided, since it may be successfully practised with portable instruments placed on the inner side of a window having a range of above 70° in altitude, or on the outer side, where they may be directed even to the zenith. "The stars which should be chosen for the purpose," Mr. Baily says, "are those which differ at least 50 degrees from each other in declination, but the nearer that difference approaches to 90 degrees, the more correct will be the results. Their right ascensions, on the contrary, must be as near as possible to each other, a circumstance which will moreover prevent the possibility of any error arising from a variation in the rate of the clock during the interval of the observations." Passing over the mode of computing the useful table of declinations with which the paper concludes, we shall copy one of the two examples of its use and application, and of the mode of operating in such cases: "On July 1, 1819, I placed my transit instrument nearly in the meridian; and in order to ascertain how much it deviated from the true meridian, I observed the two stars γ *Lyræ* and τ *Sagittarii*. The passage of the former was observed at $18^{h}.52'.37'',3$, and of the latter at $18^{h}.56'.4'',5$ siderial time. The apparent right ascensions of those stars on that day were $18^{h}.52'.9'',8$, and $18^{h}.55'.39'',7$ respectively; and their declinations were 32°. 27′ N. and 27°. 55′ S; consequently the operation will stand thus

$$\begin{array}{l} \text{Æ}^{n} = 18^{h}.\ 52'.\ \ 9,8'' \\ \text{Æ}^{s} = 18\ .\ 55\ .\ 39,7 \\ \hline {}^{d}\text{Æ} = -\qquad 3\ .\ 29,9 \end{array} \qquad\qquad \begin{array}{l} T^{n} = 18^{h}.\ 52'.\ 37,3'' \\ T^{s} = 18\ .\ 56\ .\ \ 9,9 \\ \hline {}^{d}T = -\qquad 3\ .\ 32,6 \end{array}$$

whence $(d\,T - d\,\text{Æ}) = -\,2'',7$. This value being negative, shows that the deviation is to the west: and in order to determine the quantity of the deviation, we must take the sum of the declinations (or the difference of the polar distances) of the two stars, which in this case is equal to 60°. 22′; or for the sake of round numbers, equal to 60°; and the declination of N (or the northern star) is about 32°. Consequently against the number 60, and under the column headed 32°, we shall find 1·39; which, being multiplied by $-\,2'',7$, will give $-\,3'',75$ for the deviation of the instrument in *time;* and this multiplied by 15 will give $-\,56'',3$ for the deviation in *arc* westerly."

The importance of micrometers in the practice of astronomical observation is so great, that their improvement has constituted an object of continual interest to the philosophical artist. From this uninterrupted attention, numerous suggestions have arisen; and the Rev. William Pearson, by his extensive investigations, contained in the fourth, fifth, and sixth memoirs, has contributed in a high degree to the advancement of this valuable appendage to the telescope. To detail a method of measuring small angles that has for its basis that singular property of several crystallized bodies, *double refraction*, is the purpose of the first of these essays, entitled "On the Doubly-Refracting Property of Rock Crystal, considered as a Principle of Micrometrical Measurements when applied to a Telescope." The ingenious author candidly states, that the Abbé Rochon, about the year 1783, discovered, and first made known, a method of compounding two prisms of rock crystal in such a manner that any small object seen through them appeared *double*, and the constant angular distance thus formed was made the ground-work of a micrometrical telescope. Of this original instrument, not described in any English work, an account is given; and the improvements consequent upon the discoveries of Malus, Arago, and Lenoir, are successively noticed.

But before the doubly-refracting prism can be rendered useful in measuring small angles, Dr. Pearson states that the constant angle which it measures, as viewed by the unassisted eye, must be accurately known; and also the magnifying power of the telescope as used with it; for on these data, the accuracy of the measure taken by this method entirely depends. The remainder of the memoir is accordingly devoted to a consideration of these two necessary objects. As a specimen of the manner of conducting these investigations, the second method of determining the constant angles of the prisms may be transcribed. "The prisms were now applied in succession to the small cap at the eye end of the telescope of 45·75 inches, with the view of measuring the distance between the centres of the same disc that had been used with the prisms in the cap of the object end. In the first position, all the three spider's lines were doubled; viz. the horizontal one and the two vertical ones. But turning the cap which held the prism round a little, brought the two images of the horizontal line into one, while it opened the other images or lines wider apart: a little motion given to the screw, however, soon brought the second and third lines into one strong black line, and left the first and fourth more faint, at equal distances to the right and left. In this situation I found I had obtained the measure of the angle wanted; for the second line of the first image was become coincident with the first line of the second image; and the distance of either of the extreme lines from the strong black one in the middle was the quantity of the measured angle, as indicated by the micrometer. The same thing was done at the other side of the micrometer's zero, and a mean of the two measures gave the true one without any index error. This process is as simple as accurate. When any prism is screwed into its place, the two images of the horizontal line must first be brought into one strong line, and then the two or four images of the coincident or separated lines (as the case may be) must be brought nicely into three, of which the middle one will be always much darker than either of the others by reason of there being then two images occupying the place of one. It is indeed astonishing with what degree of precision the small angle of any prism may be taken in this way; and what at first was not suspected, the micrometer indicated the same quantity, to whichever telescope it was thus applied with any prism, or even when it was detached from the telescopes altogether." Respecting the determination of the magnifying power of the telescope, the second of the objects before alluded to, the following quotation may suffice: "It has already been said, that if the constant angle of any prism be divided by the power of the telescope to which it is applied as a double image micrometer at the eye end, the quotient will be the measure of the angle, subtended by a line joining the centres of the two images of the objects observed. Therefore if the natural constant angle of any prism be divided by the measure obtained with any given power, the quotient will be that power, the constant angle being a quantity always equal to the product of any power by its corresponding measure."

(*To be continued.*)

Article X.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On the ultimate Analysis of Animal and Vegetable Substances, by Andrew Ure, MD. FRS.

On the Analysis of Sea Water, by Alexander Marcet, MD. FRS.

In this paper, the whole of which was not read, Dr. Marcet shows that the waters of the ocean do not contain mercury, as has been supposed, and that muriate of ammonia is a constant ingredient.

Article XI.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Hydriodide of Carbon.*

In the Philosophical Transactions for 1821, Mr. Faraday described a compound of iodine and olefiant gas, but he had not at that time the means of ascertaining its composition. Since that period he has obtained it in greater quantity, and analyzed it. Four grains were passed in vapour over heated copper in a green glass tube ; iodide of copper was formed, and pure olefiant gas evolved, which amounted to 1·37 cubic inch. As 100 c. i. of olefiant gas weigh about 30·15 grs. 1·37 c. i. will weigh 0·413 gr. Now 4 grs. – 0·413 leave 3·587 iodine, and 3·587 : 0·413 :: 117·75 : 13·55 nearly. Now 13·55 is so nearly the number of 2 atoms of olefiant gas that, according to Mr. Faraday, the substance may be considered as composed of

1 atom of iodine	117·75
2 atoms of olefiant gas	13·4

and is, therefore, analogous in its constitution to the compound of chlorine and olefiant gas, sometimes called chloric ether.—(Institution Journal.)

II. *General Return of Copper raised in Great Britain and Ireland in One Year ending June 30, 1822.*

	Tons.
Cornwall	9140
Ireland, and sundry parts of England, sold at Swansea	388
Devon	532
Sundry ores purchased by private contract	184
Anglesea (probably)	600
	10844

Produce of Copper Ore in Cornwall in Six Months ending June 30, 1822, from 67 mines, 52125 tons.

Particulars of the six principal mines.

	Ore.	Copper.
Consolidated Mines	6772 tons	575 tons
Dolcoath	5146	352
United Mines	3342	313
Wheal Abraham, &c.	4417	302
Pembroke	3215	251
Treskerby	2371	249
	25263	2042
61 other mines	26862	2391
	52125	4433

Produce of the ores, 8½ per cent.
Average price of copper, 108*l.* 15*s.* per ton.

III. *Attraction of Moisture by Peroxide of Copper.*

According to M. Berzelius, this oxide attracts the humidity of the atmosphere very rapidly: it is reduced so readily in hydrogen gas that if a piece be strongly heated, but not to redness, and put into a bottle of the gas, the oxide takes fire, and is reduced, and water trickles down the sides of the vessel. According to the weight lost in this mode of reduction, peroxide of copper appears to be composed of

Copper	100·0
Oxygen	25·272

(Annales de Chimie).

IV. *Influence of Green Fruits upon the Air.*

M. Theodore de Saussure has given the following as the results of his experiments on this subject:

Green fruits have the same influence as leaves upon the air both in sunshine and darkness; their action differs only in intensity, which is greatest in the leaves. During the night they cause the oxygen of their atmosphere to disappear, and they replace it by carbonic acid gas, part of which they absorb; this absorption is generally less in the open air than under a receiver.

In the dark they absorb more oxygen, when green, than when they are becoming ripe. During their exposure to the sun, they extricate, either wholly or partially, the oxygen of the carbonic acid they absorb during the night, and leave no trace of this acid in their atmosphere. Several fruits, detached from the plant, thus add oxygen gas to air which contained no carbonic acid. When their vegetation is very feeble or languid, they corrupt the air under all circumstances, but less in the sun than in darkness.

Green fruits detached from the plant, and exposed to the successive action of night and the sun, alter the air but little either in purity or volume; the slight variations observable in this respect depend either upon their greater or less power of forming carbonic acid, or upon their composition, which is modified by the degree of their maturity; thus green grapes appear to assimilate a small quantity of the oxygen of the carbonic acid which they form in the air that they vegetate in night and day; while grapes which are nearly ripe, exhibit in their atmosphere entirely during the day, the oxygen of the acid which they produced in darkness. If there be no mistake in this result, which was not strongly marked, but constant in all my experiments, it denotes the passage from the acid to the sweet state, indicating that the acidity of green fruits tends to fix the oxygen gas of the atmosphere, and that this acidity disappears when the fruit imbibes only carbon from the air or carbonic acid.

Green fruits decompose, either totally or in part, not only the carbonic acid which they have produced during the night, but also that which is artificially added to their atmosphere. When the latter experiment is made with watery fruits, and which, such as apples and grapes, evolve the acid gas slowly; they are observed to absorb* in the sun, a much greater portion of gas than an equal quantity of water would do in a similar mixture. They afterwards disengage the oxygen of the absorbed acid, and thus appear to form it in their interior. Their power of decomposing carbonic acid becomes weaker as they ripen.

During vegetation, they absorb the oxygen and hydrogen of water, depriving it of its fluid form. These results are frequently unobservable, excepting when the volume of air exceeds that of the fruit 30 or 40 times, and the heating action of the sun is much weakened: if these precautions be neglected, several fruits corrupt the air, even in the sun, by forming carbonic acid with the surrounding oxygen; but still, in the latter case, the mere comparison of their effect in the dark, with that which they produce under the successive influence of night and of the sun, shows that they decompose carbonic acid.

The differences of M. Berard's results and mine are principally derived from the circumstance of his having enclosed the fruits in a space not exceeding six or eight times their volume, which was too small, to prevent their suffering from the proximity or contact of the sides of the receiver heated by the sun. Some succulent plants resist this trial, and my results with the *cactus*, may have induced this chemist to treat fruits by the same process; but several of them require more careful management, not only than succulent plants, but even than the most delicate leaves. I think also that he ought to have nourished the fruits with a little water; the appearance of freshness which he observed in them after the experiments, might have some foundation if he had been experimenting with leaves which lose their appearance and consistence by the least drying, but it is of little value with respect to thick and fleshy fruits, which may deteriorate and lose weight, without giving any indication by mere inspection.

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* In the sun, the absorption in a mixture of 1 part of carbonic acid and 20 parts of air is equal to about two-thirds of the volume of hese fruits.

If my remarks have shown a slight error in this single point in the memoir of M. Berard, it is too rich in new and well-observed facts, to have its value diminished by it.—(Annales de Chimie et de Physique.)

V. *Chloride of Gold and Sodium.*

M. Figuier procures this compound in the following manner:

Dissolve an ounce of gold in nitro-muriatic acid, evaporate the excess of acid, and dissolve the muriate of gold in eight times its weight of distilled water; to the filtered liquor add a quarter of an ounce of decrepitated common salt, dissolved in four times its weight of water: the mixed solution is to be evaporated until it weighs only four ounces. By cooling, very regular crystals are obtained, which have the form of elongated quadrangular prisms, of a fine orange-yellow colour. No crystals of mere common salt are obtained, which happens, if a larger proportion of it be employed.

These crystals are unalterable by exposure to the air. When powdered and washed, they do not lose their colour, which would happen if they were a mere mixture of chloride of sodium and chloride of gold, for the latter is by much the most soluble.

This salt was found by analysis to be composed of

Chloride of gold	69·3
Chloride of sodium	14·1
Water	16·6
	100·0

Supposing it to be formed of one atom of chloride of gold, one atom of chloride of sodium, and eight atoms of water, M. Figuier states that its composition would be:

Chloride of gold	70·0
Chloride of sodium	13·4
Water	16·6
	100·0

(Ann. de Chimie.)

VI. *Compound of Hydrogen and Tin.*

It has been observed by Prof. Kastner, that when tin is dissolved in moderately strong muriatic acid, the hydrogen gas extricated is combined with tin, forming stanniuretted hydrogen gas. It has a peculiar and penetrating odour, and when compressed into water is dissolved in considerable quantity; it burns with a blue light, and gives off white fumes of oxide of tin: when passed into a dilute solution of gold, the powder of cassius is immediately formed, and on this account it is recommended as a test of the presence of minute portions of gold. Bismuth and zinc also are dissolved by hydrogen gas when treated in the above mode.

VII. *Frauenhofer's Experiments on the illuminating Power of the Prismatic Rays.*

By means of an ingenious photometer, M. Frauenhofer measured, with great care, the illuminating power of the different coloured spaces, and obtained results very different from those usually given. On each side of the yellow space, the light varied with very great rapidity, as appears from the following measures:

	Intensity of light.
At the 22d degree* of the red..........	0·032
At the 34th degree of the red...........	0·094
At the 22d degree of the orange.........	0·64
At the 10th degree of the yellow........	1·000
At the 42d degree of the yellow.........	0·48
At the 2d degree of the blue............	0·17
At the 16th degree of the indigo........	0·031
At the 43d degree of the violet.........	0·0056

The measures here given have no relation to the colours opposite to them, as the colours are mentioned merely to point out the position in Newton's spectrum, corresponding to the position in Frauenhofer's spectrum, where the intensity of illumination was measured. The colours in Frauenhofer's spectrum, indeed, do not correspond with those of Newton.—(Edin. Phil. Jour.)

ARTICLE XII.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

A work on the science of mineralogy is just about to make its appearance in Germany by Mr. Frederick Mohs, Professor of Mineralogy at Freyberg, and will contain the terminology, the rules of the construction of Mr. Mohs' system, and the nomenclature, the characteristic, and the descriptive part of natural history. The whole to be comprised in Two Volumes, 8vo. with plates. An English translation will appear at the same time, made under the inspection of the author, by Mr. Haidinger, who lately visited this country in company with Count Brenner.

Hogg's Treatise on the Growth and Culture of the Carnation, Pink, Auricula, Polyanthus, Ranunculus, Tulip, &c. An improved Edition, 1 Vol. 12mo.

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* The whole length of the spectrum is here supposed to be divided into 360°; the red occupying 45°; the orange 27°, the yellow 48°, the green 60°, the blue 60°, the indigo 40°, and the violet 80°.

JUST PUBLISHED.

A Practical Treatise on the Strength of Cast Iron: intended for the Assistance of Engineers, Iron Masters, Architects, &c. Also an Account of some Experiments, with an extensive Table of the Properties of Materials. By Thomas Tredgold, Civil Engineer. 8vo. Four Plates. 12*s*.

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ARTICLE XIII.

NEW PATENTS.

H. Septimus, Clapton, Middlesex, merchant, for a bolt or fastening, particularly applicable as a night-bolt.—June 4.

W. Huxham, Exeter, iron-founder, for improvements in the construction of roofs.—June 4.

H. Colebank, Broughton, in Furness Kirkley Ireleth, Lancashire, tallow-chandler, for an engine for cutting, twisting, and spreading of wicks.—June 4.

J. Barton, deputy comptroller of our mint, for a certain process for the application of prismatic colours to the surface of steel and other metals, and using the same in the manufacture of various ornaments.—June 4.

J. Frost, Finchley, Middlesex, builder, for a new cement or artificial stone.—June 11.

W. Feetham, Ludgate-hill, stove-maker, for a certain improvement on shower baths.—June 11.

ARTICLE XIV.

METEOROLOGICAL TABLE.

1822.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.	Daniell's hyg. at noon.
6th Mon.								
June 1	S W	30·24	30·23	87	47	—		
2	S E	30·25	30·24	79	47	—		
3	E	30·24	30·23	81	45	50		22
4	S E	30·23	30·20	84	51	—		24
5	N	30·21	30·20	86	50	—		
6	N E	30·21	30·21	89	49	55		
7	N E	30·21	30·14	78	45	—		
8	S E	30·14	29·99	85	52	—		12
9	S E	30·01	29·99	86	53	40		
10	S E	30·11	30·01	92	58	—	28	20
11	N	30·20	30·11	82	52	—		12
12	N	30·25	30·20	74	40	50		
13	N	30·25	30·04	83	41	—		
14	S	30·04	29·75	82	56	—	09	17
15	N	29·92	29·75	74	51	—	56	
16	E	30·25	29·92	74	42	—		
17	S E	30·25	30·24	72	41	56		
18	S	30·24	30·04	79	48	—		
19	N	30·04	30·04	80	53	—		
20	N E	30·24	30·04	70	42	—		
21	N E	30·24	30·20	74	41	54		22
22	S E	30·20	30·07	79	49	—		
23	Var.	30·07	30·07	81	61	—		
24	S W	30·14	30·07	79	58	55	05	
25	W	30·14	30·08	88	55	—		
26	S W	30·09	30·08	83	57	—		
27	N	30·12	30·09	78	53	51		
28	W	30·09	30·05	75	54	—	16	7
29	N W	30·06	30·05	67	46	20	05	
30	S W	30·12	30·05	73	50	14	—	
		30·25	29·75	92	40	4·45	1·19	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that he result is included in the next following observation.

REMARKS.

Sixth Month.—1—8. Fine: clear, and very warm. 9. A few drops of rain about six, p. m.: some lightning from 11 to 12, p. m. 10. Fine: a heavy thunder storm i the evening. 11—13. Fine: hot. 14. A shower at three, p. m. 15. Showery 16. Fine. 17. Cloudy. 18—22. Fine. 23. A slight shower about nine, a. m. wi some distant thunder. 24. Fine: a heavy shower about two, p. m. 25—27. Fin 28. Cloudy. 29. Fine: cloudy: a heavy shower about 10, p. m. 30. Cloudy an fine.

RESULTS.

Winds: N, 7; NE, 4; E, 2; SE, 7; S, 2; SW, 4; W, 2; NW, 1; Var. 1.

Barometer:	Mean height	
	For the month	30·119 inch
	For the lunar period, ending the 12th	30·167
	For 14 days, ending the 12th (moon south)	30·187
	For 12 days, ending the 24th (moon north)	30·087
Thermometer:	Mean height	
	For the month	64·683°
	For the lunar period	64·140
	For 30 days, the sun in Gemini	62·838
Evaporation		4·45 in.
Rain		1·19

*** *Daniell's Hygrometer for Fifth Month (omitted last month).*—1st, 20; 3d, 4th, 13; 6th, 6; 7th, 3; 8th, 15; 10th, 3; 11th, 24; 13th, 6; 14th, 18; 15th, 16th, 15; 17th, 21; 18th, 15; 20th, 20; 21st, 22; 22d, 10.

Laboratory, Stratford, Seventh Month, 12, 1822. R. HOWARD

ANNALS

OF

PHILOSOPHY.

SEPTEMBER, 1822.

ARTICLE I.

On the Composition of Common Verdigris.

By Richard Phillips, FRS. L. & E. &c.

IN the *Annals of Philosophy*, vol. i. p. 417 (New Series), I gave an analysis of crystallized verdigris, sometimes, but improperly, called distilled verdigris. From the experiments which I detailed, it appears to be a compound of two atoms of acetic acid, one of peroxide of copper, and three of water; or in other words, of one atom of binacetate of copper combined with three atoms of water of crystallization.

Soon after I had completed that analysis, I began an examination of common verdigris, usually termed subacetate of copper, but some difficulties occurred which induced me to discontinue the experiments. It is well known that when a small quantity of water is added to a fragment of common verdigris, it softens and swells by imbibing the water, and if more be added, a blue solution is obtained, while a portion of the verdigris remains undissolved.

It appeared to me at first probable that common verdigris might be a mixture of binacetate and subacetate of copper; the former dissolving and forming the blue solution, and the latter remaining undissolved. Upon examination, however, I could not find this to be the case, and one circumstance appeared unfavourable to such a supposition. When the verdigris in question is closely examined, it is found to contain small crystals, which, instead of being distinctly formed, and of a green colour, as is the case with the binacetate, are acicular, of a light blue colour and silky lustre.

In the state in which this compound is usually met with, it is very difficult, on account of its extreme compactness, to determine whether it consists principally of these blue crystals, or whether they are merely mixed with some other acetate, or with hydrate of copper.

During a visit to Birmingham in the latter part of last year, Mr. Badams, a manufacturer of both kinds of verdigris, showed me some light blue crystals of acetate of copper, which, he informed me, were common verdigris that had not been subjected to pressure by putting it into bags. Being desirous to ascertain the composition of these crystals, Mr. Badams was good enough to supply me with a quantity for analysis, and I shall now state the results of the experiments which I made upon them, and also upon common verdigris.

Although these blue crystals appeared to be unbroken, their size was too minute to allow of their form being determined; they are unalterable by exposure to the air, and so very light that 100 grains, when not pressed together, occupy the space of an ounce of water. When a small quantity of water is added to these crystals, they absorb it, precisely as common verdigris does; to determine the action of a large quantity of water, I put 100 grains of the crystals into a pint of it, and after occasionally agitating the mixture, the clear solution was poured off. To the insoluble residuum, half a pint of water was added; it gradually became brown, and at the expiration of three days, it had the appearance of being completely decomposed.

It appears then that the blue crystals are separable by water into a soluble acetate, and one which is insoluble, and that the latter is decomposed even by cold water.

I now attempted by direct experiment to ascertain the quantity of water which these crystals contain. With this view, 100 parts were heated in a platina crucible to the temperature of boiling water. They became of a green colour, and lost 24 parts: as, however, a portion of this loss was evidently derived from the expulsion of acetic acid, it was impossible to determine the quantity of water by direct means.

To find the quantity of acetic acid, 100 parts of the crystals were boiled in water with lime. Carbonic acid gas which had been previously sent through water, was passed into the filtered solution to precipitate the excess of lime; the solution, after being heated to expel the superfluous carbonic acid, became neutral acetate of lime, and was decomposed by carbonate of soda; the carbonate of lime precipitated, being washed and dried, weighed 28·3 parts. This experiment was repeated with but little variation in the result.

To ascertain the proportion of peroxide of copper, 100 parts of the blue crystals were heated in a platina crucible with dilute nitric acid; when the nitrate of copper formed was decomposed *by a red heat,* the peroxide left, weighed 43·2 parts. This experiment being repeated, using a flask instead of the crucible, 43·3 parts were obtained, giving a mean of 43·25 of peroxide of copper.

According to Dr. Thomson's latest experiments, the number representing hydrogen being 1, that of acetic acid is 50; and carbonate of lime being also 50, the quantity obtained in the experiments above detailed will indicate that of the acetic acid in 100 parts of the blue crystals, or 28·3 per cent. which, being added to 43·25 of peroxide of copper, will give as the composition of these crystals,

Acetic acid	28·30	
Peroxide of copper	43·25	leaving for
Water	28·45	
	100·00	

Now an atom of acetic acid being 50, that of peroxide of copper 80, and of water 9, it will appear that these blue crystals of acetate of copper are by theory composed of

		In 100 parts.
1 atom of acetic acid	50	27·17
1 atom of peroxide of copper	80	43·47
6 atoms of water	54	29·36
	184	100·00

I have already observed that these crystals are readily decomposed by water, and its effects upon the salt are sufficiently marked to merit particular notice; a small quantity of water being added to 100 grains of the crystals, the whole became a pulpy mass. When the water was increased to a pint, a blue solution was obtained, and a greenish precipitate thrown down. Upon examining this blue solution, it was found to consist of binacetate of copper, and the green precipitate of subacetate, composed of one atom of acid and two atoms of oxide. It is, therefore, evident, that in addition to the acetate and binacetate of copper already described, there exists a subacetate composed of

One atom of acetic acid	50
Two atoms of peroxide of copper 80 × 2	160
	210

When this subacetate was diluted with a further quantity of water, it became, as I have already noticed, quite brown in a few days; but whether it was totally decomposed into peroxide, or was another subsalt, I have not examined.

Having now ascertained that a compound of one atom of acetic acid and oxide of copper actually existed, I proceeded to examine whether common verdigris consists entirely of it, or is a mixture of different compounds.

For this purpose, I reduced 100 parts of French common verdigris to powder, and boiled it with excess of lime, filtered the solution, passed carbonic acid through it, decomposed it by carbonate of soda, and collected the carbonate of lime in the mode already described. The mean of two experiments carefully performed gave 29·3 of carbonate of lime, equivalent to a like quantity of acetic acid.

The quantity of oxide of copper was ascertained by boiling 100 parts of the verdigris in dilute sulphuric acid. Two parts of insoluble impurity were left, and the sulphate of copper being decomposed by heating with excess of potash, gave 43·5 of peroxide of copper. This experiment was repeated without any variation. The composition of French verdigris is, therefore, as follows:

Acetic acid	29·3
Peroxide of copper	43·5
Water	25·2
Insoluble matter	2·0
	100·0

That this is the true composition of common verdigris, and that it is essentially composed of the crystals which I have already described, was further proved by subjecting the verdigris manufactured by Mr. Badams, and in its compressed state, to a similar examination. This I found to consist of

Acetic acid	29·62
Peroxide of copper	44·25
Water	25·51
Insoluble matter	0·62
	100·00

The action of water upon both these specimens of verdigris is perfectly similar to that upon the blue crystals of acetate of copper; indeed, from the following comparative statement, it will appear, except in containing less water, occasioned by artificial drying, that when deprived of insoluble matter, the three substances resemble each other as perfectly as could be expected.

	Blue crystals.	French verdigris.	Verdigris by Tyrrell and Badams.
Acetic acid	28·30	29·3	29·62
Peroxide of copper	43·25	43·5	44·25
Water	28·45	25·2	25·51
Impurity	0·00	2·0	0·62
	100·00	100·0	100·00

M. Chaptal in his "Chimie appliquée aux Arts," mentions the silky blue crystals as forming on the surface of the plates of copper in the preparation of French verdigris. As far, however, as my knowledge extends, no analysis of them has yet been given; this is the more remarkable, because the existence of these crystals may be considered as indicative of the perfection of the manufacture.

ARTICLE II.

Experiments and Calculations for comparing the Force of a Body in Motion with Dead Weight. By Col. Beaufoy, FRS.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Bushey Heath, Stanmore, Aug.* 10, 1822.

In the last century, a remarkable difference of opinion subsisted among philosophers respecting the momentum of bodies. The English and French mathematicians maintained that the momentum was the weight multiplied into the simple velocity. The Dutch, German, and Italian philosophers, on the contrary, asserted that the momentum was the mass multiplied into the square of the velocity. This controversy appears to have been conducted with a great deal of asperity, and in some instances recourse was had to personal abuse.

The experiments to which the disputants appealed in support of their arguments not being satisfactory, has induced me to turn my attention to the subject, and endeavour to contrive an apparatus for making a series of experiments less liable to objection. How far this undertaking has been attended with success, is submitted to the readers of the *Annals of Philosophy;* but I think I have proved that a moving solid and a dead weight are not incommensurable with each other.

It is evident that when a stake is driven into the ground by a beetle, or a pile into the earth with an engine, the wood sinks lower and lower until the resistance it meets with equals the impetus of the impelling power; and then it becomes an accurate measure of the momentum of the descending ram. The only difficulty in this investigation consists in finding the resistance the pile meets with. To accomplish this purpose, I caused to be made a well-formed spiral spring, and inclosed it in a cylinder of brass. This represents the opposition of the ground to the entrance of the pile.

Through the centre of the cylinder and the middle of the inclosed helical spring, was inserted a circular brass rod; this may be considered as representing the pile to be driven, and was sufficiently long to project beyond each extremity of the cylinder. The lower extremity of the rod ran into a smaller cylinder, which was fixed to the larger one containing the spring; and this second cylinder had a slit or groove cut in the side, on which was graduated a scale of inches, and parts of an inch. In this opening slid a vernier, which divided the scale into hundredths of an inch, but which was capable of being divided by the eye into smaller fractions. The upper part of the rod (but within the larger cylinder) had a shoulder which compressed the spring when forced downwards; at the same time the lower end pushed forward the vernier to mark the degree of compression the spring underwent by the application of an external force. As it was requisite, for reasons hereafter given, that the spring when compressed should be retained in that situation; a notched piece of brass was screwed to the side of the rod, each hollow being rather more than one-tenth of an inch asunder. Into these notches, or hollows, fell a click, which offered little resistance when the rod was forced downwards; but effectually prevented its return by the action of the spring upwards. The apparatus rested on three strait and firm, but obliquely placed brass legs, and the upper part of the rod terminated with a circular piece of brass, for the purpose of receiving the impulse of the falling weight.

Another part of the machine consisted of a vertical piece of wood resting on a frame secured by screws to the floor; and on this upright by means of a mortice slid a projecting arm; the whole representing a gibbet. From the horizontal arm by means of a fine thread hung a sphere of lead, which may be considered as representing the ram of a pile engine; and the thread being cut with a sharp pair of scissors, the ball fell, which striking the brass plate, compressed the spring; the rod at the same time pushing down the vernier, the exact contraction of the spring was found by examining the scale. To find the value or effort the spring exerted, when thus compressed, the click was lifted up, the rod permitted to ascend, and the vernier kept in contact with the rod; then, as many pounds and parts of a pound were gradually placed on the top of the rod until the vernier descended and stood at the same division as it did when forced down by the impetus of the descending weight. This was considered the measure of the momentum.

The accuracy of these experiments partly depending on truly placing the rod under the falling weight, prior to each experiment, a conical plummet was hung from the arm of the gibbet, and the centre of the brass plate made to coincide with its apex.

For better adjusting the exact height of the impinging body, a circular hole was made in the projecting arm, into which was inserted a round peg, about which the opposite end of the thread that suspended the weight was twisted; and by turning the peg in the socket, after the weight had nearly gained its proper position, it was accurately adjusted.

Loading the circular plate, and by this means compressing the spring, being both tedious and troublesome, a table was formed, containing the requisite weight to press the rod every half inch.

In Table I, column 1 shows the compression of the spring in half inches. Column 2, the pounds, ounces, and drachms, that produced the effect. Column 3, the pounds, with the ounces and drachms reduced to the decimals of a pound. Column 4, the difference between the numbers in Column 3. The sum of these differences 1·708, divided by 9, gives the mean effort when compressed half an inch; and this last quotient divided by 5, and afterwards further reduced by decimal division, are the numbers placed in the remaining columns of the same Table. The weights employed in these experiments were globes of lead, the larger one weighing one pound, the smaller eight ounces; the lesser sphere was dropped from the height of 6, 12, and 18 inches; but the larger one, for want of sufficient strength in the spring, was limited to the elevation of six inches.

Column 1, of Table II, III, IV, and V, contains the number of times the ball struck the brass plate, it being requisite to continue the experiment till the effort of the spring counterbalanced the momentum of the falling weight. Column 2, the depression of the vernier after each blow; and Column 3, the difference of the numbers in Column 2. The asterisk denotes the numbers included in taking the mean.

From these experiments, a globe of lead weighing one pound avoirdupoise, and falling from the height of six inches, has an impetus of 15,143 lbs.; a leaden ball weighing half the former, or eight ounces, and falling through the respective altitudes of 6, 12, and 18 inches, acquired a momentum of 6,600, 12,899, and 19,600 pounds avoirdupoise. Half 15,143 is 7,572, which exceeds 6,600 by 972, or nearly one pound. This difference may be partly attributed to error in the experiments, and partly to elasticity, which may have a greater proportionable effect on the smaller sphere than on the larger. The resistance of the air in these experiments is so trifling as to be unworthy of notice. It is demonstrable that if a body falls through any space, and moves afterwards with the velocity gained in falling, it will describe twice that space in the time of its falling. Assuming, therefore, that a body in this latitude falls in the first second of time, a space of 193,144 inches, or 16,095 feet, by a well-known theorem $v = 2\sqrt{gs}$: g representing 16·095, the space a body falls in the first second of time; s the height of 6, 12, and 18 inches.

The uniform velocity acquired by a body falling through the spaces of 6, 12, and 18 inches, will be 5,6736, 8,0238, and 9,827 feet. On the supposition that the impetus is proportional to some power of the velocity represented by m, V and v being symbols of the velocity, I and i those of the impetus. $V^m : v^m :: I : i$; and the exponent $m = \frac{\log. I - \log. i}{\log. V - \log. v}$. By comparing the experiments made with the eight ounce weight with each other, three values of the exponent m will be obtained, viz. $m = \frac{\text{log. of } 12{\cdot}900 - \text{log. of } 6{\cdot}600}{\text{log. of } 8{\cdot}0238 - \text{log. of } 5{\cdot}6736} = 1{\cdot}9343$. $m = \frac{\text{log. of } 19{\cdot}599 - \text{log. of } 12{\cdot}900}{\text{log. of } 9{\cdot}827 - \text{log. of } 8{\cdot}0238} = 2{\cdot}0626$. $m = \frac{\text{log. of } 19{\cdot}599 - \text{log. of } 6{\cdot}600}{\text{log. of } 9{\cdot}827 - \text{log. of } 5{\cdot}6736} = 1{\cdot}9817$; the mean of these three exponents is 1·9929; which is so near the square of the velocity, that the momentum may be considered as the square of the velocity.

From the experiments with the sphere of one pound moving with an uniform velocity of 5,6736 feet in a second, was given an impetus of 15,145 pounds; and they proved the momentum to increase as the square of the elasticity. Hence may be calculated the dead weight sufficient to stop an 18 pound shot, moving with a velocity of 1000 feet in a second, $5{\cdot}6736^2$: 15·143 lbs. :: 1000^2 lbs. : 470439 : This number multiplied by 18 gives the product 8467902, the impetus of the cannon ball; which is nearly 3780 tons; a force so enormously great as hardly to be credible.

The following experiments are suggested on a larger scale, and in a different manner.

Suppose several iron pipes screwed together in the shape of an inverted syphon, the two legs parallel to each other, and united by one bent into a semicircular form; then into the shorter leg of the syphon insert a piston, with a projecting iron spindle cut into notches for a pawl to slide so as to prevent its return back after being forced down. The syphon is then to have as much water poured into the longer leg as will raise it sufficiently high to reach the bottom of the piston inserted in the other leg.

Let a pile engine be afterwards brought over the piston, and the ram permitted to fall; this striking the iron spindle will, by pressing down the piston, force up the water in the other leg; the pawl retaining the piston. The altitude of the elevated column of water may be determined by the piston's depression, and this quantity being known, with the diameter of the syphon previously measured, the weight of the water, and consequently the momentum of the falling body can be found by calculation.

It is recorded that the weight of a large battering ram, including the head, beam, iron hoops, chains, &c. weighed 41112 lbs.; and if it be presumed that the engine when employed in demolishing walls was impelled with a velocity of 12 feet per second, the momentum is equal to 277,93 tons; an impetus that equals an 18 pound cannon shot, moving at the rate of 271,15 feet in a second; consequently the force communicated to balls by gunpowder is far more efficacious in destroying buildings than the most ponderous weapons used before the invention of fire arms.

I remain, dear Sir, truly yours, MARK BEAUFOY.

TABLE I.—*Table of the Weights requisite to compress the Spring.*

1	2			3	4						
Comp.	Weight.			Weight.	Diff.	Comp.	Wght.	Comp.	Wght.	Comp.	Wght.
Inches.	lb.	oz.	dr.	lbs.	lbs.	Inches.	lbs.	Inches.	lbs.	Inches.	lbs.
0½	4	2	0	4·125	—	·1	0·342	·01	·034	·001	·003
1	6	6	0	6·375	2·250	·2	0·683	·02	·068	·002	·007
1½	8	0	0	8·000	1·625	·3	1·025	·03	·102	·003	·010
2	9	5	8	9·343	1·343	·4	1·367	·04	·137	·004	·014
2½	10	15	0	10·937	1·594	·5	1·708	·05	·171	·005	·017
3	12	8	0	12·500	1·563			·06	·205	·006	·020
3½	13	15	0	13·937	1·437			·07	·239	·007	·024
4	15	8	8	15·503	1·566			·08	·272	·008	·027
4½	17	6	0	17·375	1·872			·09	·307	·009	·031
5	19	8	0	19·500	2·125			·10	·342	·010	·034
				Mean	1·708						
				$\frac{1}{10}$	0·342						
				$\frac{1}{100}$	0·034						
				$\frac{1}{1000}$	0·003						

TABLE II.

	Experiment 1.		Experiment 2.	
Weight 1 lb.	Fall 6 inches.		Wght. 1 lb.	Fall 6 in.
1	2	3	2	3
1	0·791	Diff.	0·810	Diff.
2	1·130	0·339	1·137	0·327
3	1·430	0·300	1·428	0·291
4	1·653	0·223	1·700	0·272
5	1·907	0·254	1·917	0·217
6	2·142	0·235	2·200	0·283
7	2·269	0·127	2·398	0·198
8	2·377	0·108	2·532	0·134
9	2·509	0·132	2·666	0·134
10	2·638	0·129	2·838	0·172
11	2·740	0·102	2·910	0·072
12	2·858	0·118	3·092	0·182
13	2·964	0·106	3·298	0·206
14	3·130	0·166	3·473	0·175
15	3·240	0·110	3·516	0·043
16	3·390	0·150	3·746	0·230
17	3·501	0·111	3·934	0·188
18	3·617	0·116	*3·833	
19	3·632	0·015	3·812	
20	3·748	0·116	3·809	
21	3·764	0·016	3·854	
22	3·871	0·107		
23	3·878	0·007	15·258	
24	*3·868			
25	3·856		3·839	
26	3·871			
27	3·877			
	4)15·472			
	3·868			
Exp. 2	*3·839*			
Mean	*3·853*			

From Table I.

In.	lbs.
3·5	=13·937
0·3	= 1·025
0·05	= 0·171
0·003	= 0·010
Momentum	15·143

Table III.

	Experiment 3.		Experiment 4.	
	Weight 8 ounces.	Fall 6 inches.	Weight 8 oz.	Fall 6 in.
1	2	3	2	3
1	0·424	Diff.	0·317	Diff.
2	0·492	0·068	0·502	0·185
3	0·585	0·093	0·638	0·136
4	0·710	0·125	0·730	0·092
5	0·848	0·138	0·817	0·087
6	0·941	0·093	0·843	0·026
7	0·958	0·017	0·906	0·063
8	1·054	0·096	0·958	0·053
9	1·066	0·012	1·064	0·106
10	1·053		1·063	
11	1·058		1·060	
12	*1·068		*1·053	
13	1·067		1·054	
14	1·071		1·087	
15	1·066		1·068	
	4) ·272		4)·262	
	1·068		1·065	
Exp. 4	1·065			
Mean	1·066			

From Table I.

In.	lbs.
1 =	6·375
·06 =	·205
·005 =	·020
Momentum	6·600

Table IV.

	Experiment 5.		Experiment 6.	
	Weight 8 ounces.	Fall 1 foot.	Wght. 8 oz.	Fall 1 foot.
1	2	3	2	3
1	0·629	Diff.	0·572	Diff.
2	0·844	0·215	0·888	0·316
3	1·118	0·274	1·127	0·239
4	1·343	0·225	1·370	0·243
5	1·494	0·151	1·616	0·246
6	1·711	0·217	1·720	0·104
7	1·840	0·129	1·837	0·117
8	1·970	0·130	1·951	0·114
9	2·079	0·109	2·052	0·101
10	2·203	0·124	2·150	0·098
11	2·337	0·134	2·224	0·074
12	2·438	0·101	2·351	0·127
13	2·537	0·099	2·446	0·095
14	2·566	0·029	2·573	0·127
15	2·704	0·138	2·717	0·144
16	2·816	0·112	2·800	0·083
17	3·068	0·252	2·896	0·096
18	*3·085	0·017	3·062	0·166
19	3·085		3·120	0·062
20	3·063		3·162	0·042
21	3·078		*3·153	
	4)12·311		3·153	
			3·162	
	3·078		3·156	
Exp. 5	*3·156*		3·156	
Mean	*3·117*			

From Table I.

In.	lbs.
3 =	12·500
·1 =	·342
·01 =	·034
·007 =	·024
Momentum	12·900

TABLE V.

Experiment 7.			Experiment 8.	
Weight 8 oz. Fall 18 inches.			Weight 8 oz. Fall 18 in.	
1	2	3	2	3
1	0·769	Diff.	0·750	Diff.
2	1·161	0·392	1·079	0·329
3	1·445	0·284	1·388	0·309
4	1·758	0·313	1·730	0·342
5	1·930	0·172	2·019	0·289
6	2·239	0·309	2·264	0·245
7	2·303	0·064	2·417	0·153
8	2·589	0·286	2·528	0·111
9	2·788	0·199	2·740	0·212
10	2·918	0·125	2·797	0·057
11	3·007	0·094	2·888	0·091
12	3·427	0·420	3·187	0·299
13	3·637	0·210	3·338	0·151
14	3·788	0·151	3·437	0·099
15	3·821	0·033	3·518	0·081
16	3·874	0·053	3·942	0·424
17	4·258	0·384	4·222	0·280
18	4·406	0·148	4·456	0·234
19	4·438	0·032	4·590	0·134
20	4·604	0·166	4·657	0·067
21	4·829	0·225	4·832	0·175
22	4·829	0·000	4·884	0·002
23	4·854	0·025	4·782	
24	*5·000	0·146	4·775	
25	4·929		4·872	
26	4·952		4·858	
27	5·200		*4·953	
	4)20·081		5·270	
			4·954	
	5·020		4·980	
Exp. 7	5·039		5·039	
Mean	5·029			

From Table I.

In.	lbs.
5 =	19·500
·02 =	·068
·009 =	·031
Momentum	19·599

ARTICLE III.

Astronomical Observations, 1822.

By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

June 30. Immersion of a small star by the moon 17h 0′ 23·1″ Siderial Time.

		h	′	″	
Aug. 2. Lunar eclipse.	Beginning	10	57	43·0	Mean Time at Bushey.
	End	14	2	33·0	

Article IV.

On Sounds excited in Hydrogen Gas. By John Leslie, Esq FRSE. &c. &c.*

It is well known that the intensity of sound is diminished by the rarefaction of the medium in which it is produced. We might, therefore, expect the sound excited in hydrogen gas to be feebler than what is, in like circumstances, produced in atmospheric air. But the difference is actually much greater.

A small piece of clock-work, by which a bell is struck every half minute, being placed within the receiver of an air-pump, a successive rarefaction was produced; and after the air had been rarefied 100 times, hydrogen gas was introduced. But the sound, so far from being augmented, was at least as feeble as in atmospheric air of that extreme rarity, and decidedly much feebler than when formed in air of its own density, or rarefied 10 times.

The most remarkable fact is, that the admixture of hydrogen gas with atmospheric air has a predominant influence in blunting or stifling sound. If one half of the volume of atmospheric air be extracted, and hydrogen gas be admitted to fill the vacant space, the sound will now become scarcely audible.

These facts, I think, depend partly upon the tenuity of hydrogen gas, and partly upon the rapidity with which the pulsations of sound are conveyed through this very elastic medium. The celerity of the transmission of sound through common air is the same in every degree of rarefaction; but in hydrogen gas, it is more than three times swifter. The bell, therefore, strikes a medium which is at once thin and fugacious; fewer particles are struck, and these sooner escape from the action of the stroke. To produce undulations similar to what are excited in atmospheric air, or to cause equal reciprocations in the tide of sound, it would require the impulse to be as the square of the celerity, or 10 times greater than on common air. If this view of the matter be just, I should expect the intensity of the sound to be diminished 100 times, or in the compound ratio of its tenuity and of the square of the velocity with which it conveys the vibratory impressions.

When hydrogen gas is mixed with common air, it probably does not intimately combine, but dissipates the pulsatory impressions before the sound is vigorously formed.

It would be desirable to prosecute such observations with different gas, and at various degrees of rarefaction. But I have not yet found time, and merely throw out these hints for subsequent examination and research. John Leslie.

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* From the Transactions of the Cambridge Philosophical Society.

ARTICLE V.

Account of an Assemblage of Fossil Teeth and Bones, of Elephant, Rhinoceros, Hippopotamus, Bear, Tiger, and Hyæna, and 16 other Animals; discovered in a Cave at Kirkdale, Yorkshire, in the Year 1821: with a Comparative View of five similar Caverns in various Parts of England, and others on the Continent.

By the Rev. William Buckland, FRS. FLS. Vice-President of the Geological Society of London, and Professor of Mineralogy and Geology in the University of Oxford, &c.

(*Concluded from* p. 145.)

IN many of the most highly preserved bones and teeth, there is a curious circumstance which, before I visited Kirkdale, had convinced me of the existence of the den, viz. a partial polish and wearing away to a considerable depth of one side only; many straight fragments of the larger bones have one entire side, or the fractured edges of one side rubbed down and worn completely smooth, while the opposite side and ends of the same bone are sharp and untouched; in the same manner as the upper portions of pitching stones in the street become rounded and polished, while their lower parts retain the exact form and angles which they possessed when first laid down. This can only be explained by referring the partial destruction of the solid bone to friction from the continual treading of the hyænas, and rubbing of their skin on the side that lay uppermost in the bottom of the den. In many of the smaller and curved bones also, particularly in those of the lower jaw, the convex surface only is uniformly that which has been worn down and polished, while the ends and concave surface have suffered no kind of change or destruction; and this also admits of a similar explanation; for the curvature of the bone would allow it to rest steady under constant treading only in this position; as long as the concave surface was uppermost, pressure on either extremity would cause it to tilt over and throw the convex side upwards; and this done, the next pressure would cause its two extremities to sink into any soft substance that lay beneath, and give it a steady and fixed position. Such seems to have been the process by which the curved fragments I allude to have not only received a partial polish on the convex side only, but have been submitted to so much friction, that in several instances more than one-fourth of the entire thickness of the bone, and a proportionate quantity of the outer side of the fangs and body of the teeth have been entirely worn away. I can imagine no other means than the repeated touch of the living hyænas' feet and skin, by which this partial wearing away and polish can have been produced;* for the process of rolling by water would have made pebbles of them, or at least would have broken off the edges of the teeth and delicate points of the fractured extremities of the bone, which still remain untouched and sharp.

I have already stated that the greatest number of teeth (those of the hyæna excepted) belong to the ruminating animals; from which it is to be inferred that they formed the ordinary prey of the hyænas. I have also to add that very few of the teeth of these animals bear marks of age; they seem to have perished by a violent death in the vigour of life. With respect to the horns of deer that appear to have fallen off by necrosis, it is probable that the hyænas found them thus shed, and dragged them home for the purpose of gnawing them in their den; and to animals so fond of bones, the spongy interior of horns of this kind would not be unacceptable. I found a fragment of stags' horn in so small a recess of the cave, that it never could have been introduced, unless singly, and after separation from the head; and near it was the molar tooth of an elephant. I have seen no remains of horns of oxen, and perhaps there are none, for the bony portion of their interior being of a porous spongy nature, would probably have been eaten by the hyænas, while the outer case, being of a similar composition to hair and hoofs, would not long have escaped total decomposition. For the same reason the horn of the rhinoceros, being merely a mass of compacted hair-like fibres, has never been found fossil in gravel beds with the bones of that animal, nor does it occur in the cave at Kirkdale. I have been told that sheeps' horns laid on land for manure will be consumed in ten or a dozen years; the calcareous matter of bone being nearly allied to limestone, is the only portion of animal bodies that occurs in a fossil state, unless when preserved, like the Siberian elephant, of the same extinct species with that of Kirkdale, by being frozen in ice, or buried in peat.

The extreme abundance of the teeth of water rats has also been alluded to; and though the idea of hyænas eating rats may appear ridiculous, it is consistent with the omnivorous appetite of modern hyænas; nor is the disproportion in size of the animal to that of its prey, greater than that of wolves and foxes, which are supposed by Capt. Parry to feed chiefly on mice during the long winters of Melville Island. Our largest dogs eat rats and mice; jackalls occasionally prey on mice, and dogs and foxes will eat frogs. It is probable, therefore, that neither the size nor quatic habit of the water rat would secure it from the hyænas. They might occasionally also have eaten mice, weasels, rabbits, foxes, wolves, and birds; and in masticating the bodies of these small animals with their coarse conical teeth, many bones and fragments of bone would be pressed outwards through their lips, and fall neglected to the ground.

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* I have been informed by an officer in India that passing by a tiger's den in the absence of the tiger, he examined the interior, and found in the middle of it a large portion of stone on which the tiger reposed, to be worn smooth and polished by the friction of his body. The same thing may be seen on marble steps and altars, and even metallic statues in places of worship that are favourite objects of pilgrimage: they are often deeply worn and polished by the knees, and even lips of pilgrims, to a degree that without experience of the fact we could scarcely have anticipated.

The occurrence of birds' bones may be explained by the probability of the hyænas finding them dead, and taking them home, as usual, to eat in their den: and the fact, that four of the only five bones of birds I have seen from Kirkdale are those of the ulna, may have arisen from the position of the quill feathers on it, and the small quantity of fleshy matter that exists on the outer extremity of the wing of birds; the former affording an obstacle, and the latter no temptation to the hyænas to devour them. Two of the five bones here mentioned, in size and form, and the position of the points at the base of the quills, exactly resemble the ulna of a raven; a third approaches as closely to the Spanish runt, which is one of the largest of the pigeon tribe; a fourth bone is the right ulna of a lark; and a fifth, the coracoid process of the right scapula of a small species of duck resembling the Anas sponsor, or summer duck.*

With respect to the bear and tiger, the remains of which are extremely rare, and of which the teeth that have been found indicate a magnitude equal to the great Ursus spelæus of the caves of Germany, and of the largest Bengal tiger, it is more probable that the hyænas found their dead carcases and dragged them to the den, than that they were ever joint tenants of the same cavern. It is, however, obvious that they were all at the same time inhabitants of antediluvian Yorkshire.

In the case of such minute and burrowing animals as the mouse and weasel, and, perhaps, the rabbit and fox, it is possible that some of them may have crept into the cave by undiscovered crevices, and there died since the stoppage of its mouth; and in such case their bones would have been found lying on the surface of the mud before it was disturbed by digging: as no observations were made in season as to this point, it must remain unsettled, till the opening of another cave may give opportunity for more accurate investigation. This uncertainty, however, applies not to any of the extinct species, or to the larger animals, whose habit it is not to burrow in the ground, nor even to those of the smaller ones, *e. g.* the water rat, fragments of whose bones and teeth are found imbedded in the antediluvian stalagmite, and cemented by it both to the exterior and internal cavities of bones belonging to the hyænas and other extinct species, which, beyond all doubt, were lodged in the den before the period of the introduction of the mud. Should it turn out that since this period the cave has been accessible to foxes and weasels, it is possible that some of the birds also may have been introduced by them. The evidence of this, however, rests on a fact not yet carefully ascertained, viz. whether the bones in question were buried, like those of the extinct animals, beneath the mud, or lay on its surface; the state of one of the ravens' bones, containing stalagmite in its central cavity, seems to indicate high antiquity; and the quarryman, who was the first to enter the cave, assured me, that he has never seen a single bone of any kind on the surface, nor without digging into the substance of the mud.

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* For my knowledge of these and many other bones I have from Kirkdale, I am indebted to a careful examination and comparison of them made by Mr. Brooks, in his most valuable collection of osteological preparations. Mr. Clift also has kindly assisted me at the Royal College of Surgeons in furtherance of the same object.

As ruminating animals form the ordinary food of beasts of prey, it is not surprising that their remains should occur in such abundance in the cave; but it is not so obvious by what means the bones and teeth of the elephant, rhinoceros, and hippopotamus, were conveyed thither. On the one hand, the cave is in general of dimensions so contracted (often not exceeding three feet in diameter), that it is impossible that living animals of these species could have found an entrance, or the entire carcases of dead ones been floated into it; moreover, had the bones been washed in, they would probably have been mixed with pebbles and rounded equably by friction, which they are not: on the other hand, it is foreign to the habits of the hyæna to prey on the larger pachydermata, their young perhaps excepted. No other solution of the difficulty presents itself to me, than that the remains in question are those of individuals that died a natural death; for though an hyæna would neither have had strength to kill a living elephant or rhinoceros, or to drag home the entire carcase of a dead one, yet he could carry away, piecemeal, or acting conjointly with others, fragments of the most bulky animals that died in the course of nature, and thus introduce them to the inmost recesses of his den.

Should it be asked why, amidst the remains of so many hundred animals, not a single skeleton of any kind has been found entire, we see an obvious answer in the power and known habit of hyænas to devour the bones of their prey; and the gnawed fragments on the one hand, and album græcum on the other, afford double evidence of their having largely gratified this natural propensity: the exception of the teeth and numerous small bones of the lower joints and extremities that remain unbroken, as having been too hard and solid to afford inducement for mastication, is entirely consistent with this solution. And should it be further asked, why we do not find at least the entire skeleton of the one or more hyænas that died last, and left no survivors to devour them; we find a sufficient reply to this question, in the circumstance of the probable destruction of the last individuals by the diluvian waters: on the rise of these had there been any hyænas in the den, they would have rushed out, and fled for safety to the hills; and if absent, they could by no possibility have returned to it from the higher levels: that they did so perish on the Continent is obvious, from the discovery of their bones in the diluvial gravel of Germany, as well as in the caves. The same circumstance will also explain the reason why there are no bones found on the outside of the Kirkdale cave, as described by Busbequius on the outside of the hyænas' dens in Anatolia; for every thing that lay without on the antediluvian surface, must have been swept far away, and scattered by the violence of the diluvian waters; and there is no reason for believing that hyænas, or any other animals whatever, have occupied the den at any period subsequent to that catastrophe.

Although the evidence to prove the cave to have been inhabited as a den by successive generations of hyænas, appears thus direct, it may be as well to consider what other hypotheses may be suggested, to explain the collection of bones assembled in it.

1. It may be said, that the various animals had entered the cave spontaneously to die, or had fled into it as a refuge from some general convulsion: but the diameter of the cave, as has been mentioned before, compared with the bulk of the elephant and rhinoceros, renders this solution impossible as to the larger animals; and with respect to the smaller, we can imagine no circumstances that would collect together, spontaneously, animals of such dissimilar habits as hyænas, tigers, bears, wolves, foxes, horses, oxen, deer, rabbits, water-rats, mice, weasels, and birds.

2. It may be suggested that they were drifted in by the waters of a flood: if so, either the carcases floated in entire; or the bones alone were drifted in after separation from the flesh: in the first of these cases, the larger carcases, as we have already stated, could not have entered at all; and of the smaller ones, the cave could not have contained a sufficient number to supply 1-20th part of the teeth and bones; moreover, the bones would not have been broken to pieces, nor in different stages of decay. And had they been washed in by a succession of floods, we should have had a succession of beds of sediment and stalactite, and the cave would have been filled up by the second or third repetition of such an operation as that which introduced the single stratum of mud, which alone occurs in it. On the other hypothesis, that they were drifted in after separation from the flesh, they would have been mixed with gravel, and at least slightly rolled on their passage; and it would still remain to be shown by what means they were split and broken to pieces, and the disproportion created which exists between the numbers of the teeth and bones. They could not have fallen in through the fissures; for these are closed upwards in the substance of the rock, and do not reach to the surface.

The third, and only remaining hypothesis that occurs to me is, that they were dragged in for food by the hyænas, who caught their prey in the immediate vicinity of their den; and as they could not have dragged it home from any very great distances, it follows that the animals they fed on all lived and died not far from the spot where their remains are found.

The accumulation of these bones then appears to have been a long process going on during a succession of years, while all the animals in question were natives of this country. The general dispersion of similar bones through the diluvian gravel of high latitudes, over great part of the northern hemisphere, shows that the period in which they inhabited these regions, was that immediately preceding the formation of this gravel, and that they perished by the same waters which produced it. M. Cuvier has moreover ascertained, that the fossil elephant, rhinoceros, hippopotamus, and hyæna, belong to species now unknown; and as there is no evidence that they have at any time, subsequent to the formation of the diluvium, existed in these regions, we may conclude that the period, at which the bones of these extinct species were introduced into the cave at Kirkdale, was antediluvian. Had these species ever re-established themselves in the northern portions of the world since the deluge, it is probable their remains would have been found, like those of the ox, horse, deer, hog, &c. preserved in the post-diluvian accumulations of gravel, sand, silt, mud, and peat, which are referable to causes still in operation, and which, by careful examination of their relations to the adjacent country, can be readily distinguished from those which are of diluvian origin.

The teeth and fragments of bones above described seem to have lain a long time scattered irregularly over the bottom of the den, and to have been continually accumulating until the introduction of the sediment in which they are now imbedded, and to the protection of which they owe that high state of preservation they possess. Those that lay long uncovered at the bottom of the den, have undergone a decay proportionate to the time of their exposure; others that have lain only a short time before the introduction of the diluvian mud, have been preserved by it almost from even incipient decomposition.

Thus the phenomena of this cave seem referable to a period in which the world was inhabited by land animals, bearing a general resemblance to those now existing, before the last inundation of the earth; but so completely has the violence of that tremendous convulsion destroyed and remodelled the form of its antediluvian surface, that it is only in caverns that have been protected from its ravages, that we may hope to find undisturbed evidence of events in the period immediately preceding it. The bones already described, and the stalagmite formed before the introduction of the diluvial mud, are what I consider to be the products of the period in question. It was indeed probable, before the discovery of this cave, from the abundance in which the remains *of similar species* occur in superficial gravel beds which cannot be referred to any other than a diluvial origin, that such animals were the antediluvian inhabitants of this country; but the proof was imperfect, as it has been said they might have been drifted or floated hither by the waters, from warmer latitudes; but the facts developed in this charnel house of the antediluvian forests of Yorkshire show that there was a long succession of years in which these animals had been the prey of the hyænas, which, like themselves at that time, must have inhabited these regions of the earth; and it is in the diluvial wreck occurring in such latitudes that similar bones have been found buried in the state of grave bones over great part of northern Europe, as well as North America and Siberia. The catastrophe producing this gravel appears to have been the last event that has operated generally to modify the surface of the earth, and the few local and partial changes that have succeeded it, such as the formation of deltas, terraces, tufa, torrent-gravel, and peat-bogs, all conspire to show, that the period of their commencement was subsequent to that at which the diluvium was formed.*

It is in the highest degree curious to observe, that four of the genera of animals whose bones are thus widely diffused over the temperate, and even polar regions of the northern hemisphere, should at present exist only in tropical climates, and chiefly south of the equator; and that the only country in which the elephant, rhinoceros, hippopotamus, and hyæna, are now associated, is Southern Africa. In the immediate neighbourhood of the Cape they all live and die together, as they formerly did in Britain; while the hippopotamus is now confined exclusively to Africa, and the elephant, rhinoceros, and hyæna, are also diffused widely over the continent of Asia.

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* It was stated in describing the locality of the cave at Kirkdale, and on comparing it with the fact of its containing the remains of large and small aquatic animals, that there was probably a lake in this part of the country at the period when they inhabited it; and this hypothesis is rendered probable by the form and disposition of the hills that still encircle the Vale of Pickering.

Inclosed on the south, the west, north-west, and north, by the lofty ranges of the Wolds, the Howardian hills, the Hambleton hills, and Eastern Moorlands, the waters of this vale must either run eastward to Filey Bay, or inland towards York; and such is the superior elevation of the strata along the coast, that the sources of the Derwent, rising almost close to the sea, near Scarborough and Filey, are forced to run west and southward 50 miles inland away from the sea, till falling into the Ouse, they finally reach it by turning again eastward through the Humber. The only outlet by which this drainage is accomplished, is the gorge at New Malton; and though it is not possible to ascertain what was the precise extent of this antediluvian lake, or how much of the low districts, now constituting the Vale of Pickering, may have been excavated by the same diluvian waters that produced the gorge; it is obvious that without the existence of this gorge, much of the district within it would be laid under water; and it is equally obvious that the gorge is referable to the agency of diluvian denudation, the ravages of which have not, perhaps, left a single portion of the antediluvian surface of the whole earth, which is not torn and re-modelled, so as to have lost all traces of the exact features it bore antecedently to the operations of the deluge.

It is probable, that inland lakes were much more numerous than they are at present, before the excavation of the many gorges by which our modern rivers make their escape; and this is consistent with the frequent occurrence of the remains of the hippopotamus in the diluvian gravel of England, and of various parts of Europe. It is not unlikely that, in this antediluvian period, England was connected with the Continent, and that the excavation of the shallow channel of the Straits of Dover, and of a considerable portion of that part of the German ocean which lies between the east coast of England and the mouths of the Elbe and Rhine, may have been the effect of diluvial denudation. The average depth of all this tract of water is said to be less than 30 fathoms.

Such are the principal facts I observed in the interior of the cave at Kirkdale, and such the leading conclusions that seem to arise from them; and I cannot sufficiently lament that I was not present at its first opening, to witness the exact state in which it appeared, before any part of the surface of the mud had been disturbed.

From the description given of the state of the bones, and of the mud and stalactite that accompany them, we may extract the following detailed history of the operations that have successively been going on within the cave.

1. There appears to have been a period (and if we may form an estimate from the small quantity of stalagmite now found on the actual floor of the cave, a very short one), during which this aperture in the rock existed, but was not tenanted by the hyænas. The removal of the mud which now entirely covers the floor, would be necessary to ascertain the exact quantity of stalagmite referable to this period; but it cannot be very great, and can only be expected to exist where there is much stalactite also upon the roof and sides.

The second period was that during which the cave was inhabited by the hyænas, and the stalactite and stalagmite were still forming. The constant passage of the hyænas in so low a cave, would much interrupt this formation; as they would strike off the former from the roof and sides by their constant ingress and egress; and accordingly in some specimens of the breccia, we find mixed with the bones, fragments of stalactite, that seem to have been thus knocked off from the roof and sides of the cave, while it was inhabited by hyænas before the introduction of the mud; I have one example of a hollow stalactitic tube that lay in an horizontal position in the midst of, and parallel to, some long splinters of bone and the unbroken ulna of a rat; all these are united by stalagmite; and it is impossible that this stalactitic pipe could have been formed in any other than a vertical position, hanging from the roof or sides. In other specimens of the breccia, I have split fragments of the teeth of deer and hyæna; and in almost every portion I have seen, either of this breccia or of the antediluvian stalagmite, there are teeth of the water-rat. Mr. Gibson possesses a mass exceeding a foot in diameter, composed of fragments of many large bones, mixed with some teeth of rhinoceros and several of the larger animals, and also of rats, all adhering firmly together in a matrix of stalagmite. It did not occur to me, while on the spot, to examine whether the bottom of the cave is any where polished (like the tiger's den before alluded to), in those parts which must have been the constant gangway of the hyænas; but the universal cover of mud by which it is buried, renders it necessary that this should be removed, in order to the observation I suggest. During the formation of this stalactitic matter, no mud appears to have been introduced; and had there been any in the cave at the time while the osseous breccia was forming, it would either have excluded all access of the stalagmite to the bones, or have been mixed and entangled with it in very large proportions, forming a spongy mass, such as it does at the root of the stalagmites that lie on its surface.

The third period is that at which the mud was introduced and the animals extirpated, viz. the period of the deluge. I have already stated that the animal remains are found principally in the lower regions of this sediment of mud, which appears to have been introduced in a fluid state, so as to envelope the bony fragments then lying on the bottom of the cave: and the power of water to introduce such sediments is shown by the state of Wokey Hole, and similar caverns in the Mendip Hills, and Derbyshire, which are subject to be filled with water occasionally by heavy land floods. The effect of these floods being to leave on the floor a sediment of mud precisely similar to that which covers the bones and osseous breccia in the cave of Kirkdale. I have also mentioned that there is no alternation of this mud with beds of bone or of stalagmite, such as would have occurred had it been produced by land floods often repeated; once, and once only it appears to have been introduced; and we may probably consider its vehicle to have been the turbid waters of the same inundation that produced the diluvial gravel: these would enter and fill the cave, and there becoming quiescent, would deposit the mud suspended in them (as we see daily silt and warp deposited in quiet spots by waters of muddy rivers) along the whole bottom of the den, where it has remained undisturbed ever since. We cannot refer this mud to a land flood, or a succession of land floods, partly for the reasons before stated, and partly from the general dryness of the cave; had it been liable to be filled with muddy water, it would have been so at the time I visited it in December, 1821, at the end of one of the most rainy seasons ever remembered; but even then there were not the slightest symptoms of any such occurrence, and a few scanty droppings from the roof were the only traces of water within the area of the cavern.

The fourth period is that during which the stalagmite was deposited which invests the upper surface of the mud. The quantity of this stalagmite appears to be much greater than that formed in the two periods during, and before which, the cave was tenanted by hyænas. In the whole of this fourth period, no creature appears to have entered the cave, with the exception possibly of mice, weasels, rabbits, and foxes, until it was opened last summer, and no other process of any kind appears to have been going on in it except the formation of stalactitic infiltrations; the stratum of diluvial sediment marks the point of time at which the latter state of things began and the former ceased. As there is no mud at all on the top or sides of the cave, we have no mark to distinguish the relative quantities of stalactite formed on these parts during the periods we have been speaking of: should it, however, contain in any part a fragment of bone or tooth of any of the extinct animals, it will follow that this part was antediluvial. A further argument may be drawn from the limited quantity of post-diluvian stalactite, as well as from the undecayed condition of the bones, to show that the time elapsed since the introduction of the diluvian mud, has not been one of excessive length.

The arguments arising from the detail of facts we have been describing, are applicable to the illustration of analogous phenomena, where the evidence of their history is less complete. In our own country there are five other instances of bones similarly deposited in caverns, the origin of some of which, though not before satisfactorily made out, becomes evident as a corollary from the proofs afforded by the cave at Kirkdale: these are in Glamorganshire, Somersetshire, Derbyshire, and Devonshire.

1. The first is in the parish of Nicholaston, on the coast of Glamorganshire, at a spot called Crawley Rocks, in Oxwich Bay, about 12 miles SW. of Swansea; it was discovered in the year 1792, in a quarry of limestone, on the property of T. M. Talbot, Esq. of Penrice Castle, and no account of it has, I believe, been ever published; some of the bones, however, are preserved in the collection of Miss Talbot, at Penrice; they are as follows:

Elephant.—Three portions of large molar teeth.
Rhinoceros.—Right and left ossa humeri.
One atlas bone.
Two molar teeth of upper jaw.
Ox.—First phalangal bone of left fore foot.
Stag.—Lower extremity of the horn.
Three molar teeth.
One first phalangal bone, right leg.
Hyæna.—Two canine teeth, much worn.

These bones were found in a cavity of mountain limestone, which was accidentally intersected, like the cave at Kirkdale, in working a quarry: they have a slight ochreous incrustation, and a little earthy matter adhering to them; but are not in the least degree rolled; and the condyles of the two humeri of the rhinoceros, belonging to different individuals, have in each case been entirely broken off, as if by gnawing. The two canine teeth of hyæna (worn down to the stumps), that were found in the same cave with them, afford ground for probable conjecture as to the means by which those bones were thus broken, as well as introduced into this cave in Glamorganshire.*

2. The next case I shall mention is that of teeth and bones of elephants and other animals discovered in the Mendip Hills in cavities of mountain limestone, which were lined, and nearly filled with ochreous clay. These are preserved in the collection of the Rev. Mr. Catcott, in the City Library at Bristol. The following account of them is extracted by my friend the Rev. W. D. Conybeare, from Mr. Catcott's MS. notes: he has added also a few explanatory observations.

"The ochre pits were worked about the middle of the last century, near the summit of the Mendip Hills on the S. of the village of Hutton, near Banwell, at an elevation of from 300 to 400 feet above the level of the sea: they are now abandoned.

"The ochre was pursued through fissures in the mountain limestone, occasionally expanding into larger cavernous chambers, their range being in a steep descent, and almost perpendicular. Thus, in opening the pits, the workmen, after removing 18 inches of vegetable mould, and four feet of rubbly ochre, came to a fissure in the limestone rock, about 18 inches broad, and four feet long. This was filled with good ochre, but as yet no bones were discovered; it continued to the depth of eight yards, and then opened into a cavern about 20 feet square, and four high; the floor of this cave consisted of good ochre strewed on the surface of which were multitudes of white bones, which were also found dispersed through the interior of the ochreous mass. In the centre of this chamber, a large stalactite depended from the roof; and beneath, a similar mass rose from the floor, almost touching it: in one of the side walls was an opening about three feet square, which conducted through a passage 18 yards in length, to a second cavern 10 yards in length, and five in breadth, both the passage and cavern being filled with ochre and bones; another passage, about six feet square, branched off laterally from this chamber about four yards below its entrance; this continued nearly on the same level for 18 yards; it was filled with rubbly ochre, fragments of limestone rounded by attrition, and lead ore confusedly mixed together; many large bones occurring in the mass; among which four magnificent teeth of an elephant (the whole number belonging to a single skull) were found; another shaft was sunk from the surface perpendicularly into this branch, and appears to have followed the course of a fissure, since it is said that all the way nothing appeared but rubble, large stones, ochre, and bones: in the second chamber, immediately beyond the entrance of the branch just described, there appeared a large deep opening, tending perpendicularly downwards, filled with the same congeries of rubble, ochre, bones, &c.; this was cleared to the depth of five yards; this point, being the deepest part of the workings, was estimated at about 36 yards beneath the surface of the hill; a few yards to the west of this another similar hole occurred, in which was found a large head, which we shall have occasion presently to notice."

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* On comparing one of these humeri of the rhinoceros with a similar bone from the cave at Kirkdale, I found in each case both extremities of the bone broken or gnawed off exactly to the same point, i. e. just so far as was sufficient to extract the marrow and take off the most spongy portions of the extremities, while the parts remaining were only the hardest and most compact cylindrical portions of the centre of the bones in question.

The bones from this cavern, preserved in Mr. Catcott's cabinet in the Bristol library, are the teeth and fragments of some bones of the elephant; and similar remains of horses, oxen, and two species of stag, besides the skeleton, nearly complete, of a fox. There are also molar teeth of the hog, and a large tusk of the upper jaw. This tusk probably belonged to the head mentioned in his MS. as having been found in the pit above described, and of which the following particulars are specified:—"The head was stated by the workmen to have been about three or four feet long, 14 inches broad at the top, or head part, and three inches at the snout. It had all the teeth perfect, and four tusks, the larger tusks about four inches long out of the head, and the lesser about three inches."* The tusk now preserved is about three inches long, its enamel is fine, it is longitudinally striated, and on one side of the apex truncated and worn flat by use.

On the summit of Sandford Hill, on the east of Hutton, bones of the elephant were also, according to Mr. Catcott's MSS. discovered four fathoms deep among loose rubble. Some further detail of the bones found in the cave at Hutton are given as a note in Mr. Catcott's Treatise on the Deluge (page 361, first edition), in which he specifies six molar teeth of the elephant, one of them lying in the jaw, part of a tusk, part of a head, four thigh bones, three ribs, with a multitude of lesser bones, belonging probably to the same animal. "Besides these (he adds), we picked up part of a large deer's horn very flat, and the slough of a horn (or the spongy porous substance that occupies the inside of the horns of oxen), of an extraordinary size, together with a great variety of teeth and small bones belonging to different species of land animals. The bones and teeth were extremely well preserved, all retaining their native whiteness, and, as they projected from the sides and top of the cavity, exhibited an appearance not unlike the inside of a charnel-house."

It appears to me most probable from the description given of these bones and horns, that they were not all dragged in by beasts of prey, but some of them, at least, drifted in by water, and the presence of pebbles seems to add credibility to this conjecture.

3. Another case of fossil fragments of bone has been disco-

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* The head here described is evidently that of a hog; the account of its length being exaggerated by the workmen, from whose report alone Mr. Catcott gives the measures of it. The head itself was lost or destroyed before he had seen it.

vered by Mr. Miller, of Bristol, in a cavity of mountain limestone, near Clifton, by the turnpike gate on Derdham Down: these are not rolled, but have evidently been fractured by violence: they are partially incrusted with stalactitic matter, and the broken surfaces have also an external coating of thin ochreous stalactite, showing the fracture to have been ancient; one specimen, the property of Mr. Miller, displays the curious circumstance of a fossil joint of the horse; it is the tarsus joint, in which the astragalus retains its natural position between the tibia and os calcis; these are held together by a stalactitic cement, and were probably left in this position by some beast of prey that had gnawed off the deficient portions of the tibia and os calcis.

4. A fourth case is that of some bones and molar teeth of the elephant found in another cavity of mountain limestone at Balleye, near Wirksworth, in Derbyshire, in the year 1663; one of these teeth is now in the collection of Mr. White Watson, of Bakewell. There is, I believe, no detailed account of the circumstances under which these remains were found, further than that the cavity was intersected in working a lead mine; they might possibly have been introduced in the same manner as those at Kirkdale and Crawley Rocks.

5. The fifth and last example which I am acquainted with is that described by Sir Everard Home and J. Whidby, Esq. in the Philosophical Transactions for 1817, as discovered at Oreston, near Plymouth, by Mr. Whidby, in removing the entire mass of a hill of transition limestone for the construction of the Breakwater. This limestone is full of caverns and fissures, such as may be seen at Stonehouse and elsewhere along the edge of the cliffs; that in which the bones were found was 15 feet wide, 12 high, and 45 long, and about four feet above high water mark; it was filled with solid clay (probably diluvian mud) in which the teeth and bones were imbedded, and was intersected in blasting away the body of the rock to make the Breakwater. The state of the teeth and bones was precisely the same with that of those found at Crawley rocks, they were much broken, but not in the slightest degree rounded by attrition, and Sir Everard Home has ascertained them to belong exclusively to a species of rhinoceros. A similar discovery of teeth and bones was made in 1820, in a smaller cavern, distant 120 yards from the former, being one foot high, 18 wide, and 20 long, and eight feet above the high water mark; a description of its contents is given in the Philosophical Transactions for 1821, by the same gentlemen. It contained no stalactite, which abounds in many of the adjacent caverns. Sir Everard Home describes these teeth and bones as belonging to the rhinoceros, deer, and a species of bear.

Mr. Whidby is of opinion that neither of these caverns had the appearance of ever having had any opening to the surface, or communication with it whatever; an opinion in which I can by no means acquiesce; though I think it probable that the openings had, as at Kirkdale, been long ago filled up with rubbish, mud, stalactite, or fragments of rock reunited, as sometimes happens, into a breccia as solid as the original rock, and overgrown with grass. It is now too late to appeal to the evidence of facts, as the rock in which the cave existed is entirely removed; but the circumstances of similar caverns that have communication with the surface, either open or concealed, both in this neighbourhood, and in compact limestone rocks of all ages and formations, and in all countries, added to the identity of species and undecayed state of the animal remains which they contain, render the argument from analogy perfect, to show that the bones at Oreston are not coeval, and have only an accidental connection with the rock in the cavities of which they were found.

It by no means follows from the certainty of the bones having been dragged in by beasts of prey to the small cavern at Kirkdale, that those of similar animals must have been introduced in all other cases in the same manner; for, as these animals were the antediluvian inhabitants of the countries in which the caves occur, it is possible, that some may have retired into them to die, others have fallen into the fissures by accident and there perished, and others have been washed in by the diluvial waters. By some one or more of these three latter hypotheses, we may explain those cases in which the bones are few in number and unbroken, the caverns large and the fissures extending upwards to the surface; but where they bear marks of having been lacerated by beasts of prey, and where the cavern is small, and the number of bones and teeth so great, and so disproportionate to each other as in the cave at Kirkdale, the only adequate explanation is, that they were collected by the agency of wild beasts. We shall show hereafter, that in the case of the German caves, where the quantity of bones is greater than could have been supplied by 10 times the number of carcases which the caves, if crammed to the full, could ever have contained, they were the bones of bears that lived and died in them during successive generations.

We may now proceed to consider how far the circumstances of the caves we have been examining in England appear consistent with those of analogous caverns in other parts of the world. The history of the diluvian gravel of the Continent, and of the animal remains contained in it, appears altogether identical with that of our own; and with respect to the bones that occur in caverns, the chief difference seems to be, that on the Continent some of the caves have their mouths open, and have been inhabited in the *post-diluvial* period by animals of now existing species. Thus at Gailenreuth the great extinct bear (Ursus spelæus) occurs, together with the Yorkshire species of extinct hyæna, in a cave, the mouth of which has no appearance of having ever been closed, and which at this moment would probably have been tenanted by wild beasts, had not the progress of human population extirpated them from that part of Germany.

For a description of the cavern at Gailenreuth (which I visited in 1816), I must refer to the work of Rosenmuller, published at Weimar in 1804, in folio, with engravings of nearly all the bones composing the skeleton of the extinct bear, the size of which approached nearly to that of a horse; and for a description of the caves at Blankenburg, to an account by Esper and Leibnitz, published at Brunswick.

M. Rosenmuller says, he has never seen the remains of the elephant and rhinoceros in the same cavern with those of bears; and that he has found the bones of wolves, foxes, horses, mules, oxen, sheep, stags, roebucks, badgers, dogs, and men; * and that the number of all these is in no proportion to that of the bears. The bones of all kinds occur in scattered fragments. One entire skeleton only of the Ursus spelæus is said to have been found by Bruckmann, in a cave in the Carpathians, and to have been sent to Dresden. He adds that the different state of these bones shows that they were introduced at different periods, and that those of all the animals last enumerated, including man, are in much higher preservation than those of the bears and hyænas.

Thus it appears that the bones which are in most perfect preservation, and belong to existing species, have been introduced during the *post-diluvian* period; while the extinct bears and hyæna are referable to the antediluvian state of the earth. In corroboration of this, I found in 1820, in the collection of the Monastery of Kremsminster, near Steyer, in Upper Austria, skulls and bones of the Ursus spelæus in consolidated beds of diluvial gravel, forming a pudding-stone, and dug for building near the monastery; from which it appears that this species of bear lived in the period immediately preceding the formation of that diluvium; and the same thing has been already shown of the extinct hyæna in the gravel of France and Germany.

M. Rosenmuller states that in all the caverns he has examined, the bones are disposed nearly after the same manner; sometimes scattered separately, and sometimes accumulated in beds and heaps of many feet in thickness; they are found every where from the entrance to the deepest and most secret recesses; never in entire skeletons, but single bones mixed confusedly from all parts of the body, and animals of all ages. The skulls are generally in the lowest part of the beds of bone, having from their form and weight sunk or rolled downwards, as the longer and lighter bones were moved and disturbed continually by the living animals passing over them; the lower jaws are rarely found in contact with, or near to the upper ones, as would follow from the fact last mentioned.* They are often buried in a brown argillaceous or marly earth, as in the cases of Gailenreuth, Zahnloch, and in the Hartz, which earth, from an analysis by M. Frischman, seems to contain a large proportion of animal matter derived from the decay of the fleshy parts of the bears.

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* M. Esper has found in one of the caverns containing bears' bones, fragments of urns, which from their form were probably made at least 600 years ago.

In the caves of Gailenreuth and Mockas, a large proportion of the bones is invested with stalactite. Even entire beds, and heaps of them many feet thick, are sometimes cemented together by it, so as to form a compact breccia. Occasionally they adhere by stalactite to the sides of the cavern, but are never found in the substance of the rock itself. At Sharzfelden, and in the Carpathians, they have been found enveloped with agaric mineral (lac lunæ); they have undergone no alteration of form, but the larger bones are generally separated from their epiphyses. Their usual colour is yellowish-white, but brown where they have lain in dark-coloured earth, as at Lichtenstein. At Mockas their degree of decay is by far the greatest. Even the enamel of the teeth is far gone, and the bones are perfectly white, having lost all their animal gluten, and acquired the softness and spongy appearance, as well as colour, of calcined bones; still their form is perfect, and substance inflexible, and when struck, they ring like metallic bodies falling to the ground. These retain simply their phosphate of lime. In other caverns they are usually less decayed, but they sometimes exfoliate and crack on exposure to air, and the teeth particularly are apt to split and fall to pieces, as are also those at Kirkdale.†

M. Rosenmuller is decidedly of opinion with M. Cuvier, that the bears' bones are the remains of animals which lived and died through successive generations in the caves in which we find them; nay even that they were also born in the same caves. In proof of which he has found some bones of a bear, that must have died immediately after birth, and other bones of individuals that must have died young. This is analogous to the case of numerous teeth of young hyænas with fangs not formed; and the jaws of two that had not shed their first teeth, which I found at Kirkdale.

Most of the arguments which I have used to show that the bones in Yorkshire cannot have been accumulated by the action of one, or of a succession of floods, apply with equal force to the cave at Gailenreuth, and it is unnecessary to repeat them.

* At Kirkdale, not one skull, and few, if any, of the larger bones are found entire; for these had all been broken up by the hyænas to extract the brains and marrow; and in their strong and worn out teeth we see the instruments by which they were thus destroyed. The bears, on the other hand, not being exclusively carnivorous, nor having teeth fitted for the cracking of large bones, have left untouched the osseous remains of their own species.

† It is a curious fact, that of the numerous caves in the calcareous hills near Muggendorf, that flank the valley of the Weisent-stream, those on the north chain contain not a fragment of the bones of the Ursus spelæus, while those on the south side are full of them. This may probably be explained by supposing the mouths of the former to have been closed in the antediluvian period, and afterwards laid open by denudation.

The above description of the cave at Gailenreuth, extracted from Rosenmuller, and confirmed by my own observations on the spot, may be taken as an example of the state of the other caves on the Continent, of which it is superfluous here to say any thing further than to subjoin a list given by M. Cuvier of the most important of them, and to refer to the fourth volume of his *Animaux fossiles,* for further details taken from the authors by whom these caves have been described.

The caves alluded to are as follows:

1. That of Bauman, in the county of Blankenberg, in Brunswick, on the east border of the Hartz forest, and described by Leibnitz.

2. That of Sharzfels, in Hanover, in the south border of the Hartz, described by Leibnitz, Deluc, and Bruckmann.

Behrens, in his Hercynia Curiosa, speaks of several more in the neighbourhood of the Hartz; from most of these the bones were collected during a long course of years, and sold for their imaginary medicinal virtues under the name of Licorne.

3. The caves that next attracted attention were those of the Carpathians, and the bones found in them were at first known by the name of dragons' bones, and have been described by Hayne and Bruckmann.

4. But the most richly furnished are the caves of Franconia, described by Esper and Rosenmuller, near the sources of the Mayn, in the vicinity of Bamberg and Bayreuth, at the villages of Gailenreuth, Mockas, Rabenstein, Kirch-a-horn, Zahnloch, Zewig, and Hohen Mirchfeld.

5. A fifth locality occurs at Glucksbrun, near Meinungen, on the south border of the Thuringerwald.

6. And a sixth in Westphalia, at Kluterhoehle, and Sundwich, in the country of Mark. M. Cuvier states, that the bones found in these caverns are identical over an extent of more than 200 leagues; that three-fourths of the whole belong to two species of bear, both extinct; the Ursus spelæus and Ursus arctoideus, and two-thirds of the remainder to extinct hyænas. A very few to a species of the cat family, being neither a lion, tiger, panther, or leopard, but most resembling the jaguar, or spotted panther of South America. There is also a wolf or dog (not distinguishable from a recent species), a fox and polecat. He adds that, in the caves thus occupied, there occur no remains of the elephant, rhinoceros, horse, ox, tapir, or any of the ruminantia or rodentia. In this respect they differ materially from that of Yorkshire; but such variation is consistent with the different habits of bears and hyænas, arising from the different structure of their teeth and general organization; from which it follows, that bears prefer vegetable food to that of animals, and, when driven to the latter, prefer sucking the blood to eating the flesh, while hyænas are beyond all other beasts addicted to gnawing bones.

From this circumstance it is rendered probable, that in the caves inhabited chiefly by bears, the bones of other animals should be extremely rare. But unless there be an error in the statement of M. Deluc (Lettres, vol. iv. p. 588), that a tooth found in the cave at Scharzfels was ascertained by M. Hollman to be that of a rhinoceros; and of Esper, that large cervical vertebræ of an elephant were found by M. Frischman in the cave of Schneiderloch; it follows, that these two animals occur, though very rarely, in the caves of Germany, and they may have been introduced by the few hyænas that occasionally inhabited them; that they lived in the neighbourhood of these caves, in the period immediately preceding the formation of the diluvium, is probable, from the occurrence in it of the bones of the elephant and rhinoceros near the caves of Scharzfels and Alterstein, mentioned by Blumenbach. (Archaeologia Telluris, p. 15.)

The fact mentioned by M. Cuvier of the same hyæna being common to the caves and gravel of France and Germany, and that ascertained by myself, of the Ursus spelæus occurring in the gravel of Upper Austria, proves both these extinct species to have been the antediluvian contemporaries of the extinct elephant and rhinoceros; there is, therefore, no anachronism in finding the remains of the two latter in a den that was occasionally inhabited by such hyænas and bears.

With respect to the analogies of the diluvian sediment and the stalactite in Germany and Yorkshire, in the case of the open caves that have been disturbed and ransacked for centuries, it is hopeless to expect evidence of what was the precise state of these deposits in each individual cavern at the time it was first entered. Still there is information respecting some that have been recently discovered, which is to our purpose. It is stated, that a sediment of this kind was found on the sides and floor of the cave at Glucksbrun, near Meinungen, when it was newly opened in cutting a road in 1799, and that in all the other caverns also there is mud, but no rounded pebbles. M. Deluc, in describing the matrix in which the bones are lodged in the cave at Scharzfels, says, " le fait est donc simplement, que le sol de ces cavernes est d'une terre calcaire," " qu'en creusant cette couche molle, on en tire quantité de fragmens d'os; et qu'il s'y trouve aussi des concrétions pierreuses qui renferment des os." (Deluc, Lettres, vol. iv. p. 590.) These concretions with bones appear analogous to the stalagmitic concretions at Kirkdale, and the soft calcareous earth by which they are covered, resembles its stratum of mud. Again, the resemblance holds also in the existence both of bones and soft mud in the smallest recesses of the caverns. He says, p. 589, " Il faut en quelques endroits se trainer sur le ventre, par dessous la pierre dure pour continuer a y creuser." This is an exact description of the state of the extremities of the cave at Kirkdale at the present moment.

Leibnitz, in his description of this same cavern, has the following words to the same purpose, "Limo nigricante vel fusco infectum est solum."—(Leibnitz, Protogaea, p. 65.)

Esper thus describes the state of the floor near the entrance of one of the largest caverns at Gailenreuth. "Dans toute la contrée le terrain est marneux, mêlé avec du limon, et tire sur le jaune, mais ici on trouve une terre moins limoneuse dans une profondeur considérable. Je ne prétends pas encore la prendre absolument pour une terre animale telle qu'est sans contredit la terre qui se trouve plus bas, mais probablement elle doit y être apportée, p. 9. This again is consistent with the circumstances of the cave at Kirkdale, the mud, thus dubiously spoken of, being probably of diluvial origin, and reposing on, and being mixed with, the animal earth that had been formed before its introduction. The absence of black animal earth at Kirkdale, results from the fact of the flesh, and great part even of the bones of the animals introduced to it, having been eaten by the hyænas.

The identity of time and circumstances which I am endeavouring to establish between the German and English caverns, does not, however, depend so much on comparisons between the stalactitic matter and earthy sediments which they contain, as on the agreement in species of the animals entombed in them, viz. in the agreement of the animals of the English caves with those of the diluvian gravel of the greater part of Europe; and, in the case of the German caves, on the identity of the extinct bear with that of the diluvian gravel of Upper Austria, and the extinct hyæna with that of the gravel at Canstadt, in the valley of the Necker; and at Eichstadt, in Bavaria; to these may be added the extinct rhinoceros, elephant, and hippopotamus, which are common to gravel beds as well as caves. And hence it follows, that the period at which all these caverns were inhabited by the animals in question, was antecedent to the formation of that deposit of gravel, which it seems to me impossible to ascribe to any other origin than a transient deluge, affecting universally, simultaneously, and at no very distant period, the entire surface of our planet.

The bones found in these caverns are considered by M. Cuvier to be of older date than those of the osseous breccia, which, at Gibraltar and various places along the coast of the Mediterranean and Adriatic, occur in vertical fissures of limestone. This breccia contains fragments of bones and teeth of various ruminating and gnawing animals; that is, of ox, deer, antelope, sheep, rabbits, rats, mice; also of the horse and ass, of snakes and birds, mixed with land shells, and angular fragments of the adjacent rock; all united into a solid breccia by ochreous stalactite. The greater number of these animals agree with species that now exist, and are supposed by M. Cuvier to have fallen into the fissures in the period succeeding the last retreat of the waters. I do not see why some of them may not also have fallen in during that earlier period in which the bears occupied the caves of Germany, and the hyænas that in Yorkshire; for some of the animals found at Kirkdale seem to agree in species with those that occur in the fissures; but as they are at the same time not distinguishable from existing species, the argument arising from this resemblance is imperfect. The discovery of the extinct elephant, rhinoceros, hippopotamus, bear, and hyæna in this breccia, should it ever be made, would be decisive of the question.

For an account of the bones accumulated in these fissures, I must again refer to the works of M. Cuvier, which contain more sound and clear philosophical reasoning on the early state of habitation on our planet, and a more valuable collection of authentic facts relating to the history of its fossil animals of the higher orders, than can be found in all the books that have ever yet been written on the subject.

APPENDIX.

It was mentioned, when speaking of Gailenreuth, that human remains had been discovered there in the same cave with the bones of antediluvian animals, but that they are of comparatively low antiquity.

Three analogous cases have been noticed in this country in cavities of mountain limestone, at Burringdon, in Somersetshire, and in Glamorganshire and Caermarthenshire; and these also are attended by circumstances which indicate them to be of post-diluvian origin.

1. The discovery of human bones incrusted with stalactite, in a cave of mountain limestone at Burringdon, in the Mendip hills, is explained, by this cave having either been used as a place of sepulture in early times, or been resorted to for refuge by wretches that perished in it, when the country was suffering under one of the numerous military operations which, in different periods of our early history, have been conducted in that quarter. The mouth of this cave was nearly closed by stalactite, and many of the bones were incrusted with it. In the instance of a skull, it had covered the inside as well as the outside of the bone; and I have a fragment from the inside, which bears in relief casts of the channel of the veins along the interior of the skull. The state of these bones affords indications of very high antiquity; but there is no reason for not considering them post-diluvian. Mr. Skinner, on examination of this cave, found the bones disposed chiefly in a recess on one side, as in a sepulchral catacomb; and in the same neighbourhood, at Wellow, there is a large artificial catacomb of high antiquity, covered by a barrow, and constructed after the manner of that at New Grange, near Slane, in the county of Meath, of stones successively overlapping each other till they meet in the roof. In this were

found the remains of many human bodies. A description of it may be seen in the Archæologia for 1820.

2. Mr. Dillwyn has observed two analogous cases in the mountain limestone of South Wales; one of these was discovered in 1805 near Swansea, in a quarry of limestone at the Mumbles, where the workmen cut across a wedge-shaped fissure, diminishing downwards, and filled with loose rubbish, composed of fragments of the adjacent limestone, mixed with mould. In this loose breccia lay confusedly a large number of human bones that appear to be the remains of bodies thrown in after a battle, with no indications of regular burial; they were about 30 feet below the present upper surface of the limestone rock.

3. The other case occurred, in 1810, at Llandebie, in Caermarthenshire, where a square cave was suddenly broken into, in working a quarry of solid mountain limestone on the north border of the great coal basin. In this cave lay about a dozen human skeletons in two rows at right angles to each other. The passage leading to this cave had been entirely closed up with stones for the purpose of concealment, and its mouth was completely grown over with grass.

It is obvious, that in neither of these cases are the bones referable to so high an era as those of the wild beasts that occur in the caves at Kirkdale, and elsewhere.

P.S. As this paper was going to the press, I have been gratified to hear that my conjecture, as to the abundance of such caverns as that at Kirkdale, has been verified by the discovery of another cave (containing chambers lined with stalactite, and having on its bottom mud, and bones imbedded in the mud), in a quarry close to the town of Kirby Moorside, on the property of C. Duncombe, Esq. who has judiciously taken every precaution to secure it from injury, till some qualified person shall be present to observe, and record the undisturbed appearance presented by its interior. Should it be in my power, as I hope it may, to assist at its further opening, I shall communicate the result to the Royal Society.

It is recollected also, that about 20 years ago, another cavity containing bones was discovered on the north of Kirby Moorside, but none of them have been preserved.

Though it is probable, as I have stated, that such caverns are not uncommon, we shall cease to wonder that they are so rarely brought to light, when we consider the number of accidental circumstances that must concur to lead to such an event. 1. The existence of caverns is an accidental circumstance in the interior of the rock, of which the external surface affords no indication, when the mouth is filled with rubbish and overgrown with grass. 2. The presence of bones is another accidental circumstance, though probably not an uncommon one in the case of those caves, the mouths of which were accessible to the wild beasts that inhabited this country in the period immediately preceding the deluge. 3. A further requisite is, the intersection of one of these caves in which there happen to be bones, by a third accident, viz. the working of a stone quarry by workmen who have sufficient curiosity or intelligence to notice and speak of what they find, and this to persons who may be willing or able to appreciate, and give publicity to the discovery. The necessary concurrence of all these contingencies renders it probable, that however great may be the number of subterraneous caverns, in an inland country, very few of them will ever be discovered, or, if discovered, be duly appreciated. Those I have mentioned in Devon, Somerset, Derby, and Glamorganshire, were all laid open by the accidental operations of a quarry or mine.

May 24, 1822.—I have this day received the entire lower jaw of an hyæna from Lawford, near Rugby, in Warwickshire. It was found by Andrew Bloxam, Esq. in the same diluvial clay and gravel with the bones of elephant and rhinoceros. This is the first instance of the remains of hyæna being noticed in the diluvium of England. The animal must have perished by the same catastrophe which extirpated the hyænas, and closed the den at Kirkdale, and which swept together the remains of elephant, rhinoceros, and hyæna, in the diluvian gravel of the Continent. The support which this recent discovery gives to my arguments on the cave in Yorkshire, is too obvious to require pointing out.

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EXPLANATION OF THE PLATES.

Plate XIV.

Fig. 1. View of the mouth of the cave at Kirkdale in the face of a quarry, near the brow of a low hill.

Fig. 2. Section of the cave before the mud had been disturbed.

A. Stratum of mud covering the floor of the cave to the depth of one foot, and concealing the bones.

B. Stalagmite incrusting some of the bones, and formed before the mud was introduced.

C. C. Stalagmite formed since the introduction of the mud, and spreading horizontally over its surface.

D. Insulated stalagmite on the surface of the mud.

E. E. Stalactites hanging from the roof above the stalagmites.

Fig. 3. Ground plan of the cave, by W. Salmond, Esq. showing its extent, ramifications, and the fissures by which it is intersected.

Plate XV.

Fig. 1. Outside view of the right lower jaw of the modern Cape hyæna.

Fig. 2. Analogous portion of lower jaw of the Kirkdale hyæna, *being nearly one-third larger.*

Fig. 3. Inside view of No. 2.

Article VI.

Additional Remarks on the Influence of Moisture in modifying the Specific Gravity of Gases. By John Apjohn, MD.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Trinity College.*

In the number of the *Annals* for May last, you did me the favour of publishing some observations of mine upon the influence of moisture in modifying the specific gravities of the gases. In that paper, I gave an expression for the specific gravity of a gas saturated with moisture, and also suggested a method of determining the exact specific gravity of a gas perfectly dry. The principle upon which I proceeded, namely, that the density of steam is directly as its tension, has been called in question by Mr. Herapath, in a succeeding number of your journal, in a paper, in which he also contests the correctness of Dr. Thomson's idea of the sensible and latent heat of steam, beginning at 32, constituting a constant quantity. With this latter topic, I have no concern. But as Mr. H. conceives that he has proved from received principles that the density of steam is not simply as its tension, and as he appears to me to have by no means accomplished what he asserts, I shall briefly state the reasons which have led me to this opinion. Mr. H. has certainly adopted the most decisive method for achieving his object, for he proceeds at once to show what the true relation between them is. I hope, however, to prove, that the gentleman has fallen into an error, and that this error consists in his confounding gases and vapours, substances, as to many even of their physical properties, essentially distinct, and as to none more so than the relation existing between the density, temperature, and elasticity of each. Before I proceed, I beg to be understood as admitting that the expression $S' = S \cdot \frac{\tau'}{\tau} \cdot \frac{F + 448}{F' + 448}$ given by Mr. H. properly represents the relation between the density and tension of a permanently elastic gas, or even of steam when separated from the water which has produced it. That the following remarks may be the better understood, I shall give the steps which lead to this expression. Gases, and even steam isolated, have been found by experiment to expand the $\frac{1}{480}$th of their volume for every degree of Fahrenheit. Hence it follows, that if 480 represent the tension of any of them at 32, the tension, at any higher temperature F, will be $448 + F$. The following proportion then may be instituted. $448 + F : 448 + F' :: \tau$ the tension at $F : \frac{\tau . 448 + F'}{448 + F}$ the tension at F′. And again, since at a given temperature F′, the specific gravities are as the tensions, we shall have $\frac{\tau \,.\, 448 + F'}{448 + F}$: τ' any other tension :: s the specific gravity corresponding to the former tension : $s' = s \,.\, \frac{\tau'}{\tau} \,.\, \frac{448+F}{448+F'}$ the specific gravity corresponding to the latter. Now it being ascertained (according to Mr. H.) "by the concurrent experiments of the French and English philosophers, that, with the exception of their not being able to sustain more than a certain pressure according to the temperature, vapours are perfect gases, and follow precisely the same laws of expansion and contraction," he easily infers that the specific gravity of steam is not as its elasticity. With deference, however, to Mr. H. he has, I must say, though no doubt unintentionally, misrepresented the philosophers. It is true they have shown, that vapours apart from their respective fluids, "obey the same laws of expansion and contraction with the other gases." But the case is far different with vapours in contact with their fluids. The effects of an increment of temperature upon gases, or vapours apart from, and those same vapours in juxtaposition with their fluids, are strikingly distinct. The volume being given, the elasticity of the gas is augmented, but not its density. On the other hand, not only the elasticity, but also the density of a vapour in contact with its fluid is increased. Let us return now to the above-mentioned proportions of Mr. H. The first evidently does not apply to vapours over fluids, for the volume being given, it supposes the density also given, which in the case of vapours so situate is not the fact. The second is also without meaning here, or is at best but a trifling proposition; for what does it state? Why, that if the temperature be given, the density is as the tension. But the tension of a vapour in contact with its fluid is always the same at the same temperature, and, therefore, so must the density. Now to say that *a* varies as *b*, when neither *a* or *b* vary at all, is certainly little short of being absurd. The original proposition, therefore, namely, that the density of steam is as its tension, has not been shaken by Mr. Herapath, for the result which he arrives at, and from which he deduces *its* falsehood, does not apply to vapours in contact with their fluids. It still, however, may be doubted whether the density and tension of steam are so simply related, for Mr. H.'s failure to prove the negative does not establish the affirmative of the proposition. A few accurate experiments, by determining the specific gravity of steam at different temperatures, would enable us (as we are already possessed of tables of elasticities) to bring this law to the test of experience. Gay-Lussac indeed having already determined its specific gravity at 212, another determination would afford at least a single comparison. It will be observed that I do not any where assert *that the density* of steam is precisely as its elasticity. Mr. H.

acknowledges it to be nearly, and I confess many circumstances lead me to conclude it to be strictly the case. In the absence of proof, I do not wish to dogmatise. I shall, however, briefly advert to a circumstance which appears to me to render any but the simple relation inadmissible. It is well known that at a given temperature, the densities of gases are as the forces which compress them. From this fact, and Newton's expression for the elasticity (see any work on pneumatics), it follows that their particles repel each other with a force which varies inversely as the distance between their centres. Now if the density of steam be not as the force which compresses it, or in other words, as its tension, it must follow that its particles repel each other according to a different law, a circumstance improbable, when we consider the accordance of its expansion, when apart from water, with that of gases. This argument, however, I am not disposed to insist upon, as my principal object has been to show, that Mr. Herapath's formula does not comprehend vapours in contact with their fluids.

Your obliged humble servant,
JAMES APJOHN.

ARTICLE VII.

Observations upon D.'s Answer to C.'s Remarks upon Mr. Herapath's Theory.

(To the Editor of the *Annals of Philosophy.*)

SIR,

I AM sorry again to occupy any space in your *Annals* on the subject of Mr. Herapath's theory, but the observations of your correspondent D. require some notice from me, and will excuse, I hope, my wishing once more to trespass upon your kindness. Had he indeed confined himself to reasoning, I could without concern have left it to your readers to have determined whether or not my objections to that theory were satisfactorily answered; but by the charges he has made against me, I am obliged for my own satisfaction again to obtrude myself upon you. His manner indeed I do not complain of, as he seems to think it natural I should; he has no doubt chosen that which he thinks the most effective and convincing; and I may, therefore, with as much reason, complain of his differing from me on any other subject as on the propriety of that manner.

One of the charges to which I allude is more applicable to a moral than an intellectual deficiency; and consequently, if true, would be a disgrace, instead of a misfortune. It is contained in the following extract from D.'s letter in the *Annals* for April, p. 292. "Accuracy, it seems to me, should be rigidly adhered to in all discussions. An author should never be made to say what he has not. In more than one instance, C. has not been over delicate in this respect."

By this no doubt D. means to insinuate, that I make little scruple to state a writer's meaning or expression to be different from that which I believe it really is—an insinuation which I must take leave to assert is wholly unfounded and unjustifiable. To state that a writer means that which it is known he does not mean is as direct a falsehood, as to assert, he says, that which he does not say; and to do either, would be so degrading to any one guilty of such misconduct, as to render him unworthy of any other attention than such as might be necessary for his exposure; whether or not I have so done will best appear from an examination of what D. has offered as an instance.

"At present," says D. "I shall adduce an example which will serve as a specimen of the rest; and lest there should be any mistake or difficulty in turning to Mr. H.'s opinion, I shall place right against it one or two quotations from his first paper.

Quotations from

"C.'s Observations on Mr. Herapath's Theory, *Annals* for Dec. 1821, p. 420."	"Mr. Herapath's paper, *Annals* for April, 1821, p. 279."
"But whether the atoms be elastic, or hard, having the properties of elastic bodies which Mr. Herapath has attributed to them."	"Therefore it seemed to me that the ultimate atoms ought to possess two properties *in direct contrariety*, hardness and elasticity."

The evident meaning of the extract from my former paper is, that Mr. H. has attributed to hard bodies properties which do actually belong to elastic bodies. Now this he might have done even though he had really thought the properties of elasticity and hardness to be in direct contrariety to each other, it being sufficiently clear that however opposed he might have esteemed them to be, it is still possible that he might have erroneously attributed to the one, properties which really belong to the other. But whether the statement that he did so be correct or erroneous, the sentence does not pretend to give either Mr. H.'s expression, or his meaning; and, therefore, cannot possibly have misrepresented the one or the other. It cannot fairly be made to amount to more than an assertion, that some of the properties which Mr. H. has attributed to hard bodies in my judgment belong to elastic bodies. That the opinion expressed in the extract is really not ill founded, will appear from the following quotations:

Mr. Herapath says, "If two hard and equal balls come in contact with equal and opposite momenta, they will separate after the stroke with the same velocity with which they met."—(*Annals*, April, 1821, p. 285.)

Sir Isaac Newton says, " Bodies which are either absolutely hard, or so soft as to be void of elasticity, will not rebound from one another. Impenetrability only makes them stop. If two equal bodies meet directly in vacuo, they will by the laws of motion stop where they meet and lose all their motion, and remain at rest; unless they be elastic and receive new motion from their spring."—(Newt. Opera, vol. iv. p. 258.)

" Non-elastic bodies on their shock will adhere together, and either remain at rest, or else move together as one mass with a common velocity; or if elastic, they will separate after the shock with the very same velocity with which they met and shocked." —(Hutton's Math. Dict. in verb. Percussion, p. 215.)

These propositions from such men as Newton and Hutton (and similar might be extracted from the writings of Maclaurin, Playfair, and other philosophers of that rank) will, I hope, justify my opinion that Mr. H. did attribute to hard bodies properties actually belonging to elastic bodies. It is, however, quite clear, that no one could honestly believe those expressions amounted to " an assertion that Mr. H. makes hardness and elasticity the same;" yet so D. in a subsequent part of his paper (*Annals*, May, p. 350) ventures untruly to call it, and that for the purpose of making it appear, contrary to *the fact*, that I had asserted that which was not true.

It is, however, most extraordinary that D. in the very moment of his attack upon another for a supposed misrepresentation of the meaning of Mr. Herapath, should, with all the formality with which he has introduced the quotation from his paper, misstate his expression. There is in fact no such word as "elasticity" in the sentence which D. pretends to quote, he having substituted that word for the word " softness." Nor, I fear, can we in excess of candour attribute such a strange proceeding to accident, or oversight; since he has by his attempting in a note to excuse it, proved that he did it wilfully. It is not, however, easy to conceive, what sufficient excuse can be made for intentionally giving as a quotation from another paper that which D. knew at the time was not so.

Nor was the alteration made to accommodate the sentence to Mr. H.'s meaning; for he must have known that Mr. H. did not think hardness and elasticity to be " *in direct contrariety;*" for in a sentence the very next to one which D. has quoted on this subject, Mr. H. calls elasticity " *almost* the very opposite of hardness;" and it is evident that what he thought only " *almost* the very opposite," he could not think to be "in direct contrariety."

D.'s knowledge of Mr. Herapath's real opinion on the subject too is proved by his own note; which is as follows: " Mr. H.

has written softness, but immediately before he tells us that elasticity is nothing but an active kind of softness; and he now, therefore, uses softness instead of elasticity merely to make the contrast the stronger." By what means D. knows that Mr. H. used softness *instead of* elasticity, he has not informed us; but Mr. H. could not have wanted, nor was it possible for him to have obtained a stronger contrast than that which was "in direct contrariety;" if, therefore, he thought "softness," a stronger contrast than "elasticity," he could not have thought elasticity to be "in direct contrariety."

In stating too that the term "softness" was used for the purpose of making the contrast stronger, he admits that Mr. H. advisedly used the one word instead of the other; consequently it is evident that D. with full knowledge on the subject, attributes a word to Mr. H. not only which he did not use, but which he intentionally avoided.

Thus D. at the instant of censuring one person for a pretended misrepresentation, has, in order to give the charge an appearance of truth, intentionally misstated the actual words of another, making him say not only what he did not intend, but what he did not believe; and for this purpose has attributed to him an expression which he knew was on consideration rejected.

If D. thus misstates the expressions and meaning of Mr. H. I could hardly indulge an expectation of being differently treated. I was, therefore, little surprised subsequently to find that there is hardly a single quotation which D. pretended to make from my former paper, where he has not misstated either the words, or meaning, or both.

The first proposition of any importance to which he refers is the following: "In innumerable instances (if the words are taken in their usual sense), true conclusions may be brought out from false principles by correct reasoning. If, for instance, the errors on each side should exactly compensate each other, the result will be correct, though the foundation be erroneous." D. in quoting these sentences, omits some words, and transposes others, without marking the alterations, but as the tone and emphasis of the sentences are changed, rather than the sense, it is not of material consequence. The meaning of these sentences it would seem hardly possible to mistake. It is most evident from the whole paragraph, that it is the false principles, and the foundation only, to which errors are ascribed, and the reasoning is supposed in all cases to be correct; and it is surely unnecessary to occupy your pages in proving that it is possible to reason correctly from erroneous data. D. however, in order to raise an apparent contradiction, has assumed that I meant to attribute errors to the reasoning, at the same time too that I concluded the reasoning to be correct. "So then," he observes, "*correct* reasoning must contain *errors;* that is, I apprehend *truth* must be *error.* Of course, by parity of argument, *false* reasoning must contain *no* errors, or *error* must be *truth*, and *wrong, right*." Thus, by apprehending one piece of nonsense, and assuming "of course" another, he triumphantly concludes that there are absurdities in the propositions, of which they do not in fact contain the slightest trace. The intelligence and fairness of such observations are just equal.

His next criticism is found in the following extract: "Alluding to the loss and developement of heat in the changes of states, C. objects to Mr. H.'s theory of heat by motion, because heat may for a time become imperceptible, and again be developed without being destroyed. 'If, therefore,' says C. 'heat and motion be identical, motion cannot be destroyed, which the experience of every day tells us is untrue.' Here C. would plainly charge Mr. H.'s theory as being incompetent to explain, nay, as being repugnant to the phænomena of latent heat. Now observe 'Mr. H.'s Theory of the Changes of State and the Concomitant Phænomena,' in which the subject C. alludes to is copiously explained, was published in the *Annals* for October; C. in his 'Observations,' dated nearly a fortnight afterwards, tells us he had seen this very number of the *Annals*, and of course this very explanation, for the want of which he gravely tells the world Mr. H.'s theory is defective." However unjustifiable it may have been in D. to misquote the expressions of Mr. Herapath, yet as it was for the purpose of supporting his theory, the injury was not to Mr. H. but to D.'s own character. But in the foregoing paragraph, D. not only states that I made assertions and charges which I never did make, but even by inverted commas, as though they were literal extracts, ascribes to me expressions which I never used, and a meaning which I never intended; and that for the express purpose of raising the imputation that I had stated what was unsupported by fact. It is not true that I objected to Mr. H.'s theory of heat by motion, "because heat may for a time become imperceptible, and again be developed without being destroyed." I did not charge Mr. H.'s theory "as being incompetent to explain," or "as being repugnant to the phænomena of latent heat." I did not object, nor in any way allude, to that part of Mr. H.'s theory, however erroneous I may have thought it; consequently, I never did "tell the world that his theory was defective," for want of any explanation in relation to it. Every one of those assertions of D. both in substance and effect, are utterly untrue. This will be clearly proved by the paragraph itself, to which he refers. It is the following: "Experiment has clearly shown that caloric, or the immediate cause of heat, whatever it may be called, cannot be destroyed. However, under particular circumstances, it may become for a time imperceptible, it can be again developed, and so be shown to have continued its existence; if, therefore, heat and motion be identical, motion cannot be destroyed. This, I apprehend, the experience of every day, in addition to mathematical argument, tells us is untrue. We all every day see motion generated and destroyed. Nor can this objection be answered by a supposed difference in the nature of the motion, as we cannot even conceive of any difference in motions, except that which is made by their quantity and direction."

The reasoning contained in these observations, intended to show that the indestructibility of caloric is a strong argument to prove it cannot be merely motion, whether well founded or not, is too clear to need any further explanation; D. has not attempted to answer it, but, as I have shown, he has resorted to a method of evading its force, which intelligence and integrity would have alike disdained.

The next subject of D.'s reply is an objection to the "gaseous body of very great tenuity," which Mr. H. supposes "fills all space." The observations are not worthy of notice except as affording another instance of the kind of misrepresentation of meaning to which D. has resorted. The following is the sentence to which D.'s observations were applied: "The only proper answer to such a supposition is, 'Show this fluid to me; prove its existence by some other evidence than its being necessary to support your theory, for that argument can have little weight which founds the truth of a theory upon a supposed fluid, the existence of which fluid itself rests only upon the truth of the theory.'"

To this, D. replies: "But the oddity of this request is, 'Show me this fluid.' Surely C. does not wish Mr. H. to make this fluid visible. He does not wish, does he, Mr. H. to catch and bring to him a nameless being, a few particles of a fluid, &c." I should think D. would not wish his intelligence should be estimated so low, as to have it supposed he was incapable of perceiving that I did not mean by the term "Show," to express a wish to have the fluid rendered visible; but he must choose between such an estimation of his intellect and the estimation of his fairness, which would arise from the supposition that his observations were only applicable to a meaning which he knew I did not intend.

D. proceeds to observe, "C. speaks of Sir Isaac Newton, and insinuates to the world that Mr. H. is trying to overturn him. Except in the absolute equality of reciprocal attraction in the planets, which Newton deduced merely from analogy, and of which no proof whatever can be furnished, there is no one phænomenon in which Mr. Herapath does not perfectly agree with Newton."

D. would not have much reason to boast of Mr. H.'s modesty, if it were true, that he did only differ from Sir Isaac Newton in his opinions relating to the mutual attractions of the heavenly bodies; on those opinions are established Newton's noblest fame; nor will they, above all others, ever cease to be an honour to the age and nation in which he lived. The evidence of their truth is not merely analogy; they are still more strongly confirmed by the soundest mathematical demonstrations, and the ablest observations of astronomy. But the assertion possesses as little truth as modesty. I have already given one instance in which Mr. Herapath directly opposes Newton, where there is no relation to the reciprocal attraction of the planets; and as it respects the laws of the collision of hard bodies, it is a disagreement on the very basis of Mr. H.'s theory. And in addition, in the same paper from which D.'s extracts are taken, and to which almost the whole of his observations relate, Mr. H. does himself refer in terms to Newton's Cor. 5, of the third law of motion, and there expressly attempts to controvert it, and to prove that it is not true in cases of the collision of unequal hard bodies.—(*Annals*, April, 1821, p. 2.)

"But," says D. "since C. opposes Newton to Mr. H. I beg to ask him on what grounds he does it? Is it on the doctrine of heat?" And he then continues for the purpose of declamation, pretending to believe that I opposed Mr. H. to Newton upon that ground, although in the only two sentences in my paper in which Newton's name is mentioned, the subject of opposition is expressly mentioned to be "the doctrines of Newton in relation to the collision of hard bodies," and for the fact of that opposition, I have Mr. H.'s own authority.

D. after such introductory observations, proceeds "to examine the objections to the theory of heat by motion," nor will the examination disappoint the promise of such an introduction.

The first objection which he attempts to answer, is, where it is shown that consequences necessarily arising from the theory are contradicted by experiment; whence it is concluded that the theory itself cannot be correct. It will not be necessary to go through the reasoning to understand the kind of answer which is given to it. It was said by me in the course of the argument, "if one atom *a*, of the body A, having a greater velocity than *b*, of the body B, overtake the slower atoms, the atom *a* will lose some of its velocity, which will be communicated to the atom *b*, and thence among the other atoms of the body B. The communication of motion from the atoms of A to the atoms of B will not be compensated; for the atoms of B having less velocity than the atoms of A, will never overtake them. The motion of the atoms of B, therefore, will be increased. So that if one body A have atoms of a less magnitude than a body B with which it is in contact, but with a velocity inversely greater (that is, according to Mr. H. the bodies A and B being of the same temperatures), the momentum of the atoms (that is, the temperature of the body B) shall continually increase." D. having extracted the greater part of this proposition, says, "What becomes of the temperature of A? I do not know; C. has not told us; but I suppose as the temperature of B shall continually increase, *that of A increases too.*" It must be remembered in examining the truth of this observation, that Mr. H.'s theory, upon which this argument is founded,' and the truth of which for the purpose of deducing the consequences is assumed, considers the motion of the atoms and the heat of the body to be the same thing. And then notwithstanding it is expressly stated that part of the motion of the atoms of the body A, that is, part of the heat of the body A, is communicated to the atoms of the body B, without any compensation, he ventures to assert that I have not told him what becomes of the temperature of the body A, but that he supposes it increases. He then proceeds: "Hence we have another source of heat we did not know of before. It is only to put two bodies in contact with unequal particles, and we shall have heat generated without the aid of friction or percussion; and without chemical, galvanic, or electric action. All this results by C.'s mathematics," &c.

Here D. first states that I have not told him what becomes of the temperature of A, which, to say the least, is a mere equivocation; as I have told him what becomes of the heat of A, according to the theory which D. supports, and upon which the argument is founded; he then *supposes* that the temperature of A *increases*, without offering the slightest pretence for such a supposition, and immediately positively asserts that the absurd consequences to which that *supposition* would lead "result by my mathematics." I fear it is impossible to attribute with any reasonable probability such misrepresentations only to a want of capacity to understand the meaning of propositions so clear and intelligible; nor would a theory be worth the trouble of an examination, which rested on the arguments of an intellect capable of such mistakes. Some of the misstatements indeed are founded upon mere invention; and, therefore, could not have arisen from misapprehension; and what then must be thought of a writer capable of such perversion of truth, or of a theory requiring such support.

The arguments I formerly used to show that the consequences fairly deducible from Mr. H.'s propositions in relation to the nature of heat and temperature, are inconsistent with facts, and, therefore, incorrect, were necessarily founded on the propositions in the form and words of Mr. H. himself; what modifications he might afterwards choose to make in them, it was of course impossible I should foresee. They were mere inventions, and the same rules of philosophical argument (if there be any such) which authorised the exercise of the imagination at first, will equally justify his inventing new qualities to answer objections founded upon his former statements. But unless D. be Mr. Herapath himself, I do not see upon what grounds he can assume the same right. At all events he cannot justify making new *contradictory* propositions, yet such are assumed in D.'s reply. For instance, having assumed for argument sake, Mr. H.'s statements "that heat arises from an intestine motion of the atoms or particles, and is proportional to their individual momentum;" and that the temperature of bodies is equal when the velocities of the particle are inversely proportional to their magnitude; I concluded that "the greater atoms having less velocity than the smaller will never overtake them." Upon this D. observes: "That is not universally the case. In consequence of the mutual action of the particles, they move both in their goings and returnings swifter at some parts of their paths than at others. Generally speaking, in the exterior particles, which are those of the two bodies that come in contact, their velocities are the swiftest immediately before and after the collision; and the slowest immediately preceding and following the exterior extremity of their path. Hence, therefore, the greater particles may often move much swifter than the less; and, consequently, may frequently overtake and strike them." Thus it is assumed, that the particles have paths, to which there are extremities or limits, at which they return, and near to which they move so much slower than at other parts of their paths, that the respective velocities of the particles at the time of their collision, may be directly the opposite of that of the mean motion of the bodies to which they belong. But in the preceding page, in relation to the very same particles of the same bodies, he says, "nor can it" (the particles) "return to its own body, because the collision did not give it an inward, but merely diminished its outward motion." Now this assumption that the particle will not return to its own body after the first collision, till it comes into collision with, and receives an inward motion from, another particle of the second body, is directly contrary to the other supposition that the particles have limited paths, at the extremities of which they return of themselves.

He proceeds, "Now the outward particle which" (the particle of the body A) "next strikes, must evidently meet it with the mean motion B *b* of the particles to which it belongs." But his other assumption is, that the exterior particle of bodies near the extremities of their paths (and there alone solid bodies could come into contact if they had the supposed motions) "generally speaking," move slower than the mean motion of the body. Nor can he with propriety found the supposed mean motion upon any contemplation of mine. I have not supposed any such second collision at all; nor is it probable (if it be reasonable to use such a term in relation to such a theory) that upon the supposed facts there would be a second collision.

However contradictory each of these suppositions is to the other, it is equally opposed to facts. For if it were true that the exterior particles "which are those of the two bodies which come in contact," had, generally speaking, a slower motion at the extremity of their paths, the communication of motion (that is, according to Mr. H.'s theory, the communication of temperature) from body to body, would not depend so much upon the actual temperatures of bodies in contact, as upon the parts of the paths of the particles where the collision took place. According to the well-known facts, however, the communication of temperature depends simply upon the real temperatures of the bodies in contact. On the other hand, if it were true that the particles of bodies had such a motion as D. supposes, and would not return till they received an inward motion from collision with other particles in vacuo, where they could receive no such inward motion, the particles would altogether fly off and be dissipated; which is no less contrary to fact.

The remainder of D.'s reasoning on this subject rests upon the truth of Mr. H.'s theory of the laws of collision of hard bodies; and I shall now proceed to examine the answer D. has attempted to give to my former arguments upon that part of the subject.

D. commences his observations in the *Annals* for May last, p. 357, by attempting to show that absurd consequences would follow, according to the usually admitted theory of collision of bodies, from propositions which I have made, or admitted to be true; the reasoning in this instance will be found to be as nearly as the different kind of argument will admit, of the same nature as that upon which I have already observed. Before, he misstated the obvious meaning and expressions; here he will be found to have misstated the no less obvious consequences.

D. states, "He allows that bodies act with a force equal to their momentum, and, therefore, as one consequence, that the force with which a hard fixed plane, and a hard ball moving perpendicularly upon it, come in contact, is equal to the momentum of the ball." Again C. grants that "the intensity of the force with which two hard balls moving in opposite directions come in contact is equal to the sum of their momenta." Admitting, therefore, that the three momenta in these two cases are respectively equal, it is evident by what C. himself allows to be true, "that the intensity of the collision in the latter case is double the former." "It is on all hands allowed, I believe, in the case of perfectly hard bodies, that the changes of motion have at least the same ratio as these intensities. For instance, if a certain intensity of stroke produce a certain change of motion, double, treble, &c. that intensity will generate a double, treble, &c. change of motion." Most obviously the consequences of this reasoning is, that as the intensity of the collision in the case of the two balls coming in contact is double the intensity of the collision in the case of the one ball striking perpendicularly upon the hard fixed plane, the change of motion is also double. Consequently, if when the one ball strikes perpendicularly upon the plane, the motion of the one ball is destroyed; when the two balls come into contact, double that motion, or the motion of the two balls, is destroyed. Instead of these consequences which are so direct and conclusive, and which accord with what was stated *in my former* paper, D. proceeds: "Therefore, in the case of the hard body and plane, the change of motion in the body is the half of what C. admits to that in *either* of the two moveable bodies." I certainly never did admit, nor is it even plausibly deducible from any thing which I have stated or admitted, that the change of motion in the one body is the half of that in *either* one of the two bodies; but, on the contrary, I have stated, and, I think, proved, that the change of motion in the one body is the half of the change of motion in the two bodies. But D. continues, "Consequently if, as C. asserts, each of the two bodies just lose the whole of its motion by the stroke, the body striking on the plane will lose only half its motion; and, therefore, after the stroke, it will *proceed right through the fixed imperviable plane*, with the other half motion that remains to it!" Such consequences and observations are quite worthy of D.'s previous mode of argument.

But how the proposition, that the change of motion has the same ratio as the intensity of collision, "precisely coincides with Mr. Herapath's" reasoning, D. has not explained. Mr. H. says, "if a hard spherical body impinge perpendicularly upon a hard fixed plane, the body will after the stroke remain at rest upon the plane." (*Annals*, April, 1821, p. 284.) And he also says: "But if two hard and equal balls come in contact with equal and opposite momenta, they will separate after the stroke with the same velocity with which they met." (*Annals*, April, 1821, p. 285.) In the first case, the whole motion is said to be destroyed; but in the second, when the intensity of the contact is double, and consequently when the change of motion ought to be also double, there is no change at all, either in the quantity or direction of the motion. There is a change in the direction of the balls, equal altogether to four times the effect of the one ball being stopped by the plane, but just as much motion continues in each direction as there was before the contact.

The next extract from D.'s reply, on which it will be necessary to observe, is the following: C. says, "*that the intensity of the stroke between two bodies moving towards opposite parts is equal to the sum of their momenta;*" and, therefore, when one of them is at rest before the stroke, the intensity must be equal to the momentum of the other." The words in italics, D. has placed within inverted commas, so marking it as if an extract from my former paper; yet there is no such sentence there, nor did I ever say any thing fairly capable of such a meaning. Speaking of two hard and *equal* balls which "come in contact with *equal* and opposite momenta," I said "the intensity of the force is equal to the sum of the momenta with which both balls come in contact;" and it is a statement, of the truth of which there can be no doubt; but from that there is no rational pretence to conclude as a consequence, that "when one of them is at rest before the stroke, the intensity must be equal to the momentum of the other." The body which is at rest before the stroke, yields to the force (of course not among its parts but altogether), and consequently does not receive the whole intensity. It is evident that the intensity of the stroke, according to the sense in which D. and Mr. H. use the term, must depend not only upon the momentum of the striking body, but the resistance of the body which receives the blow. When the resistance is equal to the whole force of the striking body, there the body struck receives the whole momentum; but in proportion as the resistance is less, the motion received by the resisting body is also less. The general proposition, however, which D. attributes to me, I never laid down, and his statement that I did so is absolutely false. Having, however, ascribed to me an assertion which I never made, he derives from it a consequence equally unsupported by fact. "But C. tells us," D. says, "the one body after the stroke remains at rest on the plane; therefore, the other body striking the quiescent one likewise remains at rest after the stroke." When the moving body strikes the hard fixed plane, the resistance is equal to the momentum of the moving body; but the resistance of the quiescent body is not equal to that momentum; and it cannot rationally be contended, that because when the resistance is equal to the momentum, the body remains at rest after the stroke; therefore, when the resistance is less than the momentum, the body also remains at rest. Yet D. not only assumes that it is so, but insinuates that it is a conclusion of mine; although my former paper contains nothing from which any such inference can fairly be drawn; and he knows that I have endeavoured to support the laws of collision of bodies which have been laid down by former mathematicians, by which the consequences are totally different. That D. was aware of this is evident from what follows in his paper. "Now," says D. in the sentences immediately succeeding that which I have just quoted, "though this agrees with Mr. H.'s theory, it is decidedly at variance with the old. The old theory makes the two bodies after the stroke to go on together, and hence the collision deprives the striking body of only part, not of the whole, of its motion. C. has consequently embraced views in direct opposition to the theory he means to advocate." It is certainly extraordinary that any writer should venture to make such wilful misstatements. I can only expose them. I cannot descend to apply to them the only names which would be their appropriate designation. I must leave them to that disgust which every honourable mind must feel on perceiving them.

I must, however, consider the length to which I am led by exposing these misstatements one by one, and I shall pass on to that which D. would call demonstration, having put the supposed reasoning in the form of mathematical propositions.

"If two perfectly hard and equal balls at rest be similarly struck by two other perfectly hard balls moving with equal momenta, the intensities of the strokes are equal." (Prop. A. *Annals*, May 1822, p. 260). The only material part of the reasoning by which this proposition is attempted to be supported is the following: "All the bodies being absolutely hard, the strokes are mere impulses which are begun and finished with the very commencement of the contacts, and are, therefore, equally smart with respect to duration under every velocity. Hence the velocities of the moving bodies have no effect on the intensities of the strokes." Mr. H. has stated in the *Annals* for April, 1821, p. 284, "that all the strokes between perfectly hard bodies have no duration, and are thence equally smart." If this be true, as it undoubtedly is, the strokes are equally smart with respect to duration under every momentum, and consequently it may, with just as much reason, be concluded, that the momenta of moving bodies have no effect on the intensities of the strokes.

But if the two similar hard balls which are supposed to be struck, instead of being quiescent, were moving with equal velocities, then Mr. H. himself does in a proposition which D. has adopted (*Annals*, April, 1822, p. 294), in effect clearly admit, that notwithstanding the strokes would be equally smart with respect to duration, yet the velocities of the striking bodies would have an effect upon the intensities of the strokes. "If," say they, "a hard body overtake and strike another hard body moving with less velocity in the same right line, the first body will after the stroke continue its course with the same velocity which the other body had before, and the second body will acquire from the stroke a momentum equal to the difference of the velocities of the bodies drawn into the mass of the first body." According to this proposition, if a hard body A, with a mass as 4, and a velocity as 6, that is, with a momentum as 24, overtake another hard body B, with a mass as 5, and a velocity as 3, B will acquire a momentum by the stroke $=\overline{6-3}\times 4=12$. But if the body B moving with the like velocity be overtaken by another body, C, having the same momentum as A, but having its mass as 2, and its velocity as 12, the momentum gained by B will be $\overline{12-3}\times 2=18$. In D.'s proposition, the bodies which receive the stroke are supposed to be quiescent, and in that of Mr. H. they are supposed to be moving; that difference, however, cannot affect the argument of D, which is founded solely upon the fact that the strokes are equally smart with respect to duration, and this is alike in both cases. I do not, however, allow that Mr. H.'s proposition is correct, further than as it admits that the difference in the velocities of bodies having equal momenta has an effect in the collision of hard bodies; but it serves to show the inconsistencies in the theory itself, and very rarely indeed it is, that there are not such inconsistencies in a theory which is itself inconsistent with truth.

That the difference of the velocities of hard bodies having equal momenta has an effect in their collision with hard quiescent bodies, will readily appear upon examination. If a hard moving body A, strike a hard quiescent body B, in the lines of their centres of gravity, the quiescent body yields to the stroke, and this it must do lessening A's motion, and increasing its own, until it shall have acquired a velocity equal to that of A. When B moves with a velocity equal to that of A, it is evident that A will cease to act upon it. This effect in hard bodies is produced instantaneously. These things being premised, and they are too self-evident to require further illustration, the effects of the difference in the velocities may be easily made evident by numbers. Thus if a hard body A having a mass as 8, and a velocity as 6, and consequently a momentum as 48, strike in the line of their centres of gravity a hard quiescent body B, having also a mass as 8, B will not have acquired a velocity equal to that of A until A has communicated to it motion as 24; when both A and B will have a velocity as 3. But if another body C, having the same momentum as A, say 48, but having its mass as 4, and its velocities as 12, strike B when quiescent in a similar manner, B will not have acquired a velocity equal to that of C until it has received motion as 32; when C and B will both have a velocity as 4. The quantity of motion altogether is, in both instances, the same after the stroke as before, there being no motion destroyed by the collision; but in one case the velocity acquired by B is as 3; in the other as 4. In the first case after the stroke, the whole momentum 48 is divided by the whole mass of A and B, or $8 + 8 = 16$, making the velocity as 3, and the momentum of B $8 \times 3 = 24$; in the second case, the momentum 48 is divided by the whole mass of B and C, or $8 + 4 = 12$, making the velocity of B as 4, and its momentum $8 \times 4 = 32$. But the intensity of the stroke must be in proportion to the quantity of motion acquired by B, its resistance to the stroke being greater in proportion as it was required to attain greater velocity. Though, therefore, the bodies A and C, having equal momenta, would be capable of giving strokes of equal intensity where the whole motion was expended; in the cases supposed, as the quantity of motion communicated is different, so the intensity of the strokes is different.

It will sufficiently appear from the foregoing observations, that it was not from any difficulty in answering a similar theorem in Mr. H.'s paper in the *Annals* for April, 1820, that I passed it over with many others of the same kind, but because having shown enough to prove that the theory itself was erroneous, I thought it unnecessary to trace out every error which it contained. When, therefore, D. says, that " C. descended for the purpose *of suiting his* own views to an artful omission of it," he makes an assertion which is wholly untrue, and does me the injustice of so estimating my conduct by his own, as to think that meanness possible to me, of which he has shown himself so capable.

D.'s next proposition is so entirely founded upon the first as to require no particular notice, but in order further to confirm them both, he makes quotations from Hutton, Playfair, and Emerson, which, he says, are "perfectly compatible" with his theory. That this is true can be easily imagined, as no one will doubt that there are many sentences in those authors which, having little or no relation to the question, cannot be said to be incompatible with it. Thus from Playfair, "Bodies that have equal quantities of motion have equal forces or equal powers to produce motion." But the question here is not whether they have equal powers when their whole power is exerted, but whether when the body struck yields to the blow, the whole motion is communicated; that is, whether the whole power is actually exerted. Again, "the *velocities being equal*, a double mass will strike with a double force, a triple with a triple force, and so on." (Hutton's Courses, vol. ii. p. 132.) But what has this to do with Mr. H.'s proposition, "that *the velocities* of the moving bodies have no effect on the intensities of the strokes" But his reference to Maclaurin is more singular. "Maclaurin's Fluxions," D. says, "in which I believe his views of collision are expounded, I have not by me. If I had, I should probably be able to give another amusing specimen of C.'s knowledge of names instead of things." If D. will refer to Maclaurin's "Account of Newton's Philosophical Discoveries," p. 184, et seq. he will find that he maintains "that in the actions of perfectly hard or inflexible bodies upon one another," "as there is no spring nor any force to separate them, they must go on together after their collision as if they formed one body."

It would, however, be endless to make extracts to this effect from all the other writers referred to; I have already done so in relation to some of them. But it is evident from other statements in his paper, that D. knows the fact that every one of these authors from whom he has made these quotations, do, in their works, state propositions in relation to the very point in question, directly contradictory to his theory, yet upon these quotations alone, D. in effect assumes, what he must know to be perfectly untrue, that Playfair and Hutton do not maintain those laws of collision of hard bodies which I have attempted to support." (*Annals*, May, 1822, p. 368.)

This, however, is not the only disingenuous use he makes of these quotations, as will appear from the next extract, which contains a difficulty or paradox, as he calls it, which, as he states, has perplexed him a little. "Let a perfectly hard ball A, moving with any velocity *a*, strike in the line of its motion another perfectly hard ball B at rest, then, by the old theory, the motion of B after the impulse, or the motion it acquires by the stroke $= A a - \frac{A a}{A+B} A = \frac{A a B}{A+B}$; and in any other parallel case, the motion acquired by the same B at rest $= \frac{A' a' B}{A' + B}$. Now by the views in the quotations I have made from Hutton, Playfair, Emerson, and C. himself, it is evident that if the momenta $A a$ and $A' a'$ were equal, the intensities of the strokes and the momenta due to the body B after the strokes would be equal. That is $\frac{A a B}{A+B} = \frac{A' a' B}{A' + B}$, or $A = A'$, however unequal the value of A and A′ may be."

A moment's consideration will show, that this apparent absurdity arises from another assumption made by D. without any reason, by which he attributes to the writers referred to, opinions which he knows they do not hold, and consequences which the very proposition he himself ascribes to them contradicts. That no such inference as that which D. has drawn from the quotations is fairly deducible from them, or was intended by the authors, is evident, not only from the quotations themselves, but by what the authors have written in other parts of their works. For it is still true that "bodies act with a force eqnal to their momentum," although neither the force nor momentum can fairly be measured by the effects of their collisions on bodies which yield to the stroke; and that this was the opinion of those writers, D. knew at the time he attributes the contrary, not to the quotations only, but to their views. Thus he has said before, "The old theory makes the two bodies go on together; and hence the collision deprives the striking body of only a part, not of the whole of its motion." (*Annals*, May, 1822, p. 358.) And one of the propositions which he has introduced for the purpose of controverting the old theory, is to show that the velocity of the striking body has no effect on the collision if the momenta are equal, (Ibid. p. 360.) I have already shown that the momentum of the body struck which is at rest before the stroke is *affected* by the velocity of the striking body, though other things are equal, but the proposition itself sufficiently proves the truth of the old opinions, and D.'s knowledge of them. Thus he says that the velocity of B after the stroke is $= \frac{A a B}{A+B}$, and in any other parallel case, $B v = \frac{A' a' B}{A' + B}$. But the momentum $A a = A' a'$, and B is the same in both cases; therefore, $A a B = A' a' B$. If then A be greater than A′; $A + B$ must be greater than $A' + B$, and consequently $A a B$ divided by the greater $A + B$ must be less than $A' a' B$ divided by the lesser; that is, $\frac{A a B}{A+B}$ is less than $\frac{A' a' B}{A' + B}$. When, therefore, D. states, that by the old theory, B v after the stroke is $\frac{A a B}{A+B}$, and in parallel cases = $\frac{A' a' B}{A' + B}$, he must have known that the writers who supported it did not think the momenta due to the body B after the strokes would be equal. It was, however, unnecessary in order to show the mode by which this difficult paradox was raised, to do more than refer to the paragraph itself; where he ventures to attribute to Hutton, Playfair, and Emerson, the belief that when A a = A′ a', then $\frac{A a B}{A+B} = \frac{A' a' B}{A' + B}$, however unequal A may be to A′; that is, that Hutton, Playfair, and Emerson, believed that equal quantities divided by unequal quantities produced equals.

The next proposition is the one stated by Mr. Herapath in the *Annals* for April, 1821, p. 287, with its form a little altered. "If a perfectly hard ball strike another perfectly hard ball at rest in the line described by the centre of gravity of the former, the striking body will remain at rest after the impulse, and the other will proceed in the same right line in which the former was moving, and with the same momentum."

"From this," Mr. H. has himself stated, "it follows that a body in a state of free and perfect quiescence, however small it might be, will destroy the motion of another body however large, and however great its momentum." Whether or not such propositions are not self evidently untrue, I must leave to the judgment of your readers; it is certainly impossible to exaggerate them. It will not, however, be difficult to show the fallacy in the reasoning offered in support of this proposition, nor will it, I apprehend, occasion surprise that it should be found to rest on assumptions as unfounded as those which have already been exposed.

"All that I require," says D, "for demonstrating this proposition is, *that the intensity or force of percussion be the same as, or equal to, the motion generated; and that the force of percussion be proportional to the generating momentum.* Without adverting to the preceding propositions, each of these postulates is admitted in the quotations I have made from the authors C. has quoted against Mr. Herapath." I have already made some observations on the meaning which D. has endeavoured to apply to those quotations, which are equally applicable to the postulates said to be deduced from them, upon which the reasoning in support of this proposition rests. For the generating momentum must evidently be the momentum which generates motion; that is, the momentum expended in producing motion, and consequently when the body struck yields to the stroke, the generating momentum will not be the whole momentum of the striking body. Although, therefore, the momenta of the striking bodies may be equal, the momenta expended in producing motion in other bodies at rest; that is, the generating momenta, may be unequal.

This has been so fully explained before, and is in itself so evidently true, that it would not have been again repeated, but that the whole of the argument in support of the proposition depends upon an assumption of D. that if the momenta of the striking bodies be equal, the generating momenta and the momenta of the bodies struck must be also equal. I have already shown the fallacy of those propositions and reasoning, by which D has attempted to prove that the velocity of the striking body has no effect upon the motion of the body struck, if the momenta of the striking bodies are equal; but in the support of this proposition, D. has not rested upon them, but instead has relied upon the postulates before mentioned. He then proceeds, "Let B, B′, be two perfectly hard and equal balls at rest, and let A, A′, be any two other perfectly hard balls striking respectively B, B′, according to the conditions of the proposition. Let also a, a', be the velocities of A, A′, before the strokes, so that $A\,a = A'\,a'$. Then if b, be the velocity of B after the strokes, and b' that of B′, we have $B\,b = B'\,b'$ and $b = b'$." Upon this assumption that $b = b'$ rests the whole of the reasoning supporting this proposition. I have, however, already shown that this is not the case unless $A = A'$, and consequently $a = a'$; for, as before shown, the velocity of the body struck depends upon the velocity of the striking body, and consequently $B\,b$ may differ from $B'\,b'$ to any extent less than $A\,a$. Having assumed without sufficient reason as a consequence of his postulates that $b = b'$, he proceeds to show that if it be true, and A′ has any velocity after the stroke, "the body A′ which cannot move faster than B′, because it comes behind it, might nevertheless have a greater velocity in the same direction, which is absurd." I readily admit that if it be assumed that $b = b'$, whatever may be the magnitudes of A, A′, this absurdity will follow; but this only shows that the assumption is not founded in truth; and consequently that if A be not equal to A′, then $B\,b$ shall not be equal to $B'\,b'$. But D. concludes, not that $B\,b$ is not equal to $B'\,b'$ unless $A = A'$, but that "A, A′, must remain at rest after the impulses, and consequently the bodies B, B′, proceed with the momenta $A\,a$, $A'\,a'$, respectively." That this conclusion is not warranted by the premises is sufficiently evident from the preceding observations. It was not, however, possible that the proposition should be proved by the argument *ad absurdum*, as no absurdity could be greater than the proposition itself which it was produced to prove.

The next proposition (Prop. D) is a repetition of part of Mr. Herapath's Cor. 3, Prop. 2 (*Annals*, April, 1821, p. 285), with a little variation of terms. "If two perfectly hard and equal balls come in contact, when moving with equal momenta in the same right line towards opposite parts, the intensity of the stroke as felt by each body in a direction opposite to that in which it was moving is equal to the sum of the momenta of the two, or twice the momentum of either one before the stroke." That bodies act with a force equal to their momentum, is a maxim which D. has repeatedly and triumphantly quoted, and momentum is the quantity of motion in a given direction. It is also quite clear that neither of the balls can themselves act in a direction opposite to that in which they are moving. The utmost intensity of force, therefore, with which either of the balls can act, is its own momentum; and that only in the direction towards which it moves. The acting force is necessarily the same at the time of the collision as before; and consequently at the instant of collision each ball acts with a force equal to its own momentum in the direction towards which it moves; and as both balls are moving in opposite directions, they each act with a force equal to their own momentum in a direction opposite to the direction of the other ball. The intensity of the collision, therefore, is the sum of the momenta of the two, but the force in each direction is the momentum of each one; and consequently "the intensity of the stroke as felt by each body in a direction opposite to that in which it was moving," is equal to the momentum of one ball, and not the momenta of two; for if they acted in each direction with a force equal to the momenta of two balls, it is evident the whole force would be doubled by the collision, which is impossible.

D. professes to demonstrate the proposition from the principles admitted in the whole theory, and he commences by stating truly, that "By the old theory, if a hard body A, having the velocity of a, strike another hard equal body A′ at rest, the motion communicated to A′ by the impulse is $\frac{A a}{2 A} A = \frac{A a}{2}$." This he properly treats as the intensity of the stroke, and uses it as such in his reasoning. But in the same argument in which he uses this as correct, he states, and assumes that he has proved, that "when one of the bodies is at rest," "the intensity of the stroke on each is equal to the momentum of the moving body." I have already shown that the latter statement is not true; but if it were, the former could not be so; and the reasoning can little deserve the term of strict mathematical induction, which assumes in its support as true two propositions quite inconsistent with each other; namely, that the intensity of the stroke is equal to $\frac{A a}{2}$, or half the momentum of A, and also equal to the momentum of the moving body, or the whole momentum of A. It is, however, worthy of the corollory which he founds upon it, but which has already been sufficiently refuted. "Hence," he says, "the two equal bodies after the impulse recede towards the parts whence they came, with the same momenta they had before they met."

"In the theory of motion rightly understood," says Maclaurin, in his Account of Newton's Philosophical Discoveries, p. 130, "the same laws that serve for comparing, compounding, or resolving motions, are likewise observed by pressures; that is, the powers that generate motion or tend to produce it; and it adds no small beauty to this theory of motion that both observe the same laws." Accordingly many of the laws of collision of bodies are afterwards exhibited by Maclaurin from the effects of pressure. In formerly observing, therefore, upon Mr. H.'s theory, I exhibited the incorrect consequences which were deducible from his reasoning on the laws of motion in a sentence similar to the corallary just quoted, by an instance of its effect in a case of pressure. "Thus if a man push with all his strength against a wall, say with a force as 10, action and reaction being equal, the wall resists with a force as 10, exactly in a similar manner to the fixed plane in Mr. H.'s proposition. If instead of the wall there be an opposing active force, another person, for instance, pushing against the first with an exactly equal force, the effect to the first will be just the same as the wall, and neither person will be able to move the other. But by Mr. Herapath's reasoning, each person would be acted on in a direction opposite to that towards which he pushed, by a force equal to twice the force of either one; that is, with a force as 20; and consequently both must be pushed backwards; a conclusion notoriously contrary to fact. And yet this is the reasoning by which are to be overturned, in one short page, the doctrines of Newton, Maclaurin, Hutton, Playfair, and innumerable other mathematicians, in relation to the collision of hard bodies; the first principles of which too are as nearly as possible self-evident." Upon this, D. observes, "These sentences, as far as I understand them, distinctly charge Mr. H. with confounding pressure with impulse." Certainly no understanding can be worse than one which chooses to misunderstand, and no other could derive such a charge from those sentences. He adds afterwards, "C. tells us that the pushing case I have just quoted which (with how much truth the reader may judge from the counter quotations), he informs the world, is Mr. Herapath's, is that, by which it is intended by Mr. H. that the doctrines of Newton," &c. "are to be overturned, in relation to the collision of hard bodies." I will only observe upon this, that the extract is all that I ever said on the subject; and it may be thence ascertained whether, when D. said that I charged Mr. H. "with confounding pressure with impulse," that I informed the world that "the pushing case," as he calls it, was Mr. Herapath's, and that I told them it was by that by which it was intended by Mr. H. that the doctrines of Newton, &c. were to be overturned, his assertion was not absolutely untrue. His motive in the assertion may be gathered from his insinuation that what I said was not accordant with truth.

I have now, I believe, examined all that is offered in the form of *reasoning* in D.'s papers. Had it indeed been reasoning, however able or severe it might have been, and however difficult to have been answered, that examination would have given me much pleasure. The mental effort required to meet a powerful argument, though great, is invigorating to the mind, and healthful; and gives it that tone and elastic energy which is no inconsiderable enjoyment; but the toil of dissecting and exposing a vast mass of misstatement and misrepresentation, though less difficult to accomplish, is merely laborious, fatiguing, and disgusting; and I fear the exposition will be found so by your readers. There still, however, remain one or two topics which D. has used for declamation, which will claim a few observations.

The first which I would notice is the boast that Mr. H. has compared his theory with so many experiments, and has predicted the phenomena of so many new and untried cases. Probably the credit which is claimed for Mr. H. in his prophetic character may not be readily granted, as long as the cases remain new and untried. It is, however, by no means extraordinary, that he should be able by his theory plausibly to explain many phenomena. Seriously to publish any hypothesis which was evidently incompetent to account for any of the phenomena of nature, would prove the writer not foolish, but insane; it is, therefore, to be expected, that every theory should afford an explanation of some class of experiments or observations. But that which may properly be demanded of it is, that it should besides be consistent with all the phenomena of nature; for if its truth be clearly contradicted by any one fact, that is sufficient to prove its incorrectness. In my former paper, I pointed out many cases in which facts were inconsistent with the theory; and in this, I have endeavoured to show that they still remain unexplained. But Mr. H. himself admits that his theory opposes conclusions drawn by other writers, though the observations on which they are founded are exceedingly numerous. Thus he does not hesitate to conclude, that if two in volume of hydrogen unite with one in volume of oxygen to form water, the atoms of oxygen will be double in number those of hydrogen. (*Annals*, June, 1821, p. 403.) Yet that conclusion is opposed by almost all the ablest chemical writers.

The manner in which the coincidence between the theory and those experiments with which it accords is produced is so singular, that it will deserve a few moments' examination. "On the supposition," says Mr. H. "that mercury and water are homogeneous fluids, I have found from the best experiments I can procure, that the ratio of the numeratoms of mercury and water is about equal to that of 1 to 2; and the ratio of the magnitudes of the particles equal to about that of 27 to 1; and, therefore, the ratio of their diameters, supposing them similar, about that of 3 to 1. This greater numeratom of the water is indicated by the mean temperature of the mixture of equal parts of mercury and water always being in favour of the temperature of the water, and the excess of magnitude in the particles of mercury by its less disposition to be affected in volume by changes of temperature." Thus it appears that Mr. H. pretends to ascertain the proportionate number of atoms by the mean temperature of the bodies on their mixture, as determined by experiment; and it having been so determined that if a given volume of mercury at the temperature of 100° Fahr. be mixed with an equal volume of water at the temperature of 40°, the temperature of the mixture is about 60°, and consequently that the effect of the water upon the temperature in proportion to that of mercury is as 2 to 1 nearly; that is assumed by Mr. H. to be the proportionate number of atoms. Mr. H. then proceeds, "Taking these numbers for correct, I find that if a given volume of mercury at the temperature of 100° Fahr. be mixed with an equal volume of water at the temperature of 40°, the temperature of the mixture should be $59\frac{1}{2}$°; by Dr. Henry, it is 60°. And if the same temperatures be taken, but the water be put at the higher, and the mercury at the lower temperature, the mixture should be at $79\frac{1}{2}$°; Dr. Henry says it is nearly 80°." Thus it is first assumed that if upon the mixture of equal quantities of mercury at 100°, and water at 40°, the resulting temperature is 60°, the numeratom, as Mr. H. calls it; that is, the proportionate number of atoms in the water in comparison with those in the mercury shall be as 2 to 1. And the comparison of Mr. H.'s theory with experiment consists in reasoning back again, that if the numeratom be as 2 to 1, then if a given quantity of mercury at 100° be mixed with an equal quantity of water at 40°, the resulting temperature ought to be nearly 60°. That is, if it be true that if the resulting temperature be as 60°, the numeratom must be as 2 to 1, then if the numeratom be as 2 to 1, the resulting temperature shall be as 60°. So that if you will tell Mr. H. what will be the resulting temperature of a mixture of two fluids having certain previous temperatures, he will by his theory again tell you the very same, and will also calculate what will be the temperature of a mixture of the same fluids mingled at other temperatures. This mode of reasoning will doubtless give results very accurately coinciding with experiments, but as it is merely reasoning in a circle, it can tend very little to prove the truth of the theory, however long a list may be furnished of such facts.

Another topic to which D. frequently refers, with much apparent self gratulation, is the opinions of other philosophers, and chiefly that of Sir I. Newton. To him he refers more than a dozen times, but only once for the purpose of making a quotation in confirmation of the theory, and that once he draws an inference which the next sentence would have shown was incorrect, and which is directly contradicted by other parts of his works. With what justice he claims the support of several other philosophical writers to whom he has referred, the extracts which I have already given will sufficiently show. With respect to Sir I. Newton's opinions also, I have already proved by extracts from his works, that on the laws of collision, they directly, both in words and meaning, contradict Mr. Herapath's. Even, therefore, if Newton had positively stated it as his opinion that there did exist such a gravific medium as Mr. H. speaks of, and that he really considered it to be proved that heat was only motion, yet as Mr. H.'s laws of collision of hard bodies is at the very basis of his theory, there would still exist a difference in relation to all that is peculiar to Mr. Herapath's philosophy. The manner, however, in which Newton suggests these peculiar thoughts on heat and gravity is so striking an illustration of the distinction which should be made in the statement of hypotheses and facts, and offers so singular an instance of the modesty of his exalted mind, that I cannot refuse myself the pleasure of making some extracts.

"But," says Maclaurin, speaking of Sir I. Newton, in his Account of his Philosophical Discoveries, p. 9, "while he was thus demonstrating a great number of truths, he could not but meet with hints of many other things that his sagacity and diligent observation suggested to him, which he was not able to establish with equal certainty, and as these were not to be neglected but to be separated with care from the others, he, therefore, collected them together, and proposed them under the modest title of queries."

It is in those queries, and in what he calls "*Cogitationes variæ*," that are contained those speculations of Newton on the causes and nature of heat and gravity, to which D. refers. But the manner in which he suggests them affords no pretence to consider them his opinions. Thus in the advertisement to that part of his works, in which the "Question" relating to gravity is published (Newt. Opera, vol. iv), he says, "And to show that I do not take gravity for an essential property of bodies, I have added one question concerning its cause, choosing to propose it by way of a question, because I am not yet satisfied about it for want of experiments." And in the question itself, speaking of the objections made to his opinion of gravity, because he cannot account for the causes, he says, "Later philosophers banish the consideration of such things out of natural philosophy, feigning hypotheses for explaining all things mechanically, and referring other causes to metaphysics; whereas the main business of natural philosophy is to argue from phenomena without feigning hypotheses, and to deduce causes from effects till we come to the very first cause, which certainly is not mechanical." In his letter to the Hon. Mr. Boyle (Ibid. p. 385), he says, "The truth is, my notions about things of this kind are so indigested, that I am not well satisfied myself about them; and what I am not satisfied in, I can scarce esteem fit to be communicated to others, especially in natural philosophy, where there is no end of fancying." And he then adds, "I shall set down my apprehensions in the form of suppositions." He concludes the same letter: "But by what has been said, you will easily discern whether in these conjectures there be any degree of probability, which is all I aim at. For my own part, I have so little fancy to things of this nature, that had not your encouragement moved me to it, I should never, I think, have thus far set pen to paper about them. What is amiss, therefore, I hope you will the more easily pardon."

The contrast which is thus afforded by the style of Newton to the manner in which Mr. Herapath and D. have written on the same subject, though exceedingly striking, will occasion no surprise to those who are accustomed to look for modesty and simplicity from minds in proportion as they are elevated and superior; and to expect that by how much experimental and philosophical truth is habitually contemplated with a clear and lucid perception, by just so much will these "conjectures," these "feigned hypotheses," these "fancies," as Newton calls them, be esteemed doubtful and worthless.

There are many other parts of D.'s papers which it will be perceived I have not thought worth notice. When, for instance, he over and over again mockingly repeats without any sensible application or meaning, phrases which I formerly used; when too he asserts that I am "unacquainted with one of the commonest of Newton's ideas," speaks of my "conclusions too absurd to be entertained by any other person," "ridiculous conclusions," "temerity," "folly," "absurdity," "presumption," "quibbling;" recommends me to avoid "equivocation," "subterfuge," "paltry attempt to evade," &c. with many other such insinuations and expressions, I have thought such things not deserving an answer; they only degraded the writer, if they were not indeed to be expected as the natural style and manner of one capable of the wilful misstatements and misrepresentations which I have exposed. Very many other similar misstatements and misrepresentations I have passed over without observation, where they were not interwoven with the propositions offered as answers to what I had previously written; I have shown enough to guard his readers against receiving as true, without examination, any of his assertions, however positively made; and the occupation of exposing them is too unpleasant and disgusting not to be avoided as much as it can be done with propriety.

With respect to the author of these papers, I certainly will not choose to attribute them to Mr. Herapath himself. I am aware that Mr. H. has been misled into a manner of attack upon what he calls the "illiberal opposition" from members of the Royal Society, and the "absurdities and strange paralogies" of Mr. Tredgold, which will give some countenance to the supposition that he might have been tempted to indulge in any longer *paper, in the* more liberal use of those terms not usual in philosophical controversies, but which are contained in the papers of D. Nor can it be unobserved that there is in these papers an apparent most intimate acquaintance with every part of the theory of Mr. H. both published and unpublished, and of the meaning, and even secret motives of the expressions and omissions in Mr. H.'s former papers, and at the same time an unusually energetic and triumphant interest in his philosophical opinions. These things may probably induce many persons to do Mr. H. the injustice to ascribe the papers to him, and, perhaps, therefore, he may think it worth while publicly to disown them; but for myself, having traced in them so many other unfounded assumptions, I can easily admit that these circumstances should be added to the number. The contrary too is not so easily conceived. Though indeed it is neither extraordinary nor unpardonable that a writer, having with no inconsiderable labour prepared a new theory in an important branch of natural philosophy, should be induced to value it rather more highly, perhaps than its merits would warrant, and be led by a zeal and energy in its support, to use language not suited nor usual in philosophical discussions; it is not easily to be imagined that any one who feels within him any pulse of honourable ambition, to distinguish himself in the scientific discoveries and controversies of the age, should almost at his very outset stoop to such a course of wilful misstatement and misrepresentation as D.'s papers exhibit, even to the extent of giving in inverted commas as the literal expressions of a writer, what was never written, meant, or thought by him. Such conduct must necessarily wither all his hopes in their very opening, by rendering it impossible for any person of honourable feeling to continue a correspondence with him.

I must indeed still think that Mr. Herapath has mistaken the path to philosophical science, in departing from experiment and observation, as the foundation of his opinions, and resting them on certain supposed properties of bodies, the knowledge of the existence of which is not deduced from the examination of phenomena, but springs from the imagination; contenting himself, if the theory be so framed, as to accord with some one considerable class of facts. Such was not Newton's mode of philosophical discovery. "Quicquid enim ex phænomenis non deducitur hypothesis vocanda est; et hypotheses, seu metaphysicæ, seu physicæ, seu qualitatum occultarum, seu mechanicæ, in philosophia experimentali locum non habent." (Newt. Opera, vol. iv. p. 493.) "The main business of natural philosophy is to argue from phænomena without feigning hypotheses," and when once the inductive philosophy is departed from, and the imagination, instead of fact and observation, is made the basis of theory, "there is no end of fancying." But however much it may be necessary that Mr. H. should change his course of philosophical thought and study before he can generally attain among scientific men that rank as a philosopher to which he seems to aspire, his supporter D. has much more to change in his manner and style of writing, and his integrity as a controversialist, before he can deserve that any further arguments or observations of his should be regarded with any other feeling than contempt.

I remain, yours, &c. C.

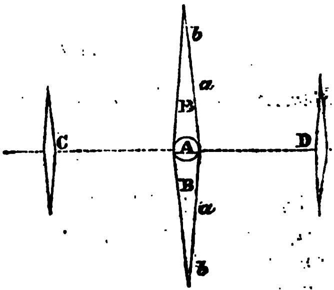
Article VIII.

Lunar and Solar Phenomena seen at Toula, in Russia. By Mr. Longmire.

(To the Editor of the *Annals of Philosophy*.)

SIR, *Whitehaven, Aug.* 8, 1822.

On Feb. 18, 1819, O. S. at about eight o'clock, p. m. the moon being nearly full, and nine degrees above the horizon, and the night still and very clear, with hard frost, there appeared near the moon six perpendicular spires of light; every two of which had a common base in the middle, and in a horizontal line drawn through the moon's centre. The whole formed three perpendicular elongations of light; the middle one being on the moon, and the others, at 9° on each side of it. The point of the upper spire at the moon was 16° above the horizon, and the inverted spire extended to the ground. The other elongations were half the length and breadth of the middle spires; whose base was equal to the apparent diameter of the moon; which, where these spires were seen, was enlarged one-fifth.



The colour of the elongation at the moon was a light yellow, unbroken for two thirds of the length; but nearer both points, it consisted of perpendicular blue streaks, somewhat lighter than the sky. Perpendicular blue streaks formed the other elongations. The stars shone through the streaks, and the sky was seen in the spaces between them

I have attached a sketch to this paper in which A is the moon, B the spires on it, C D the other spires; the parts *a b* are light yellow, and the parts *c c*, with the whole of the spires C D, are blue.

These very beautiful phenomena were visible to a spectator in the town, but not in the country adjoining it. The heat from the houses seemed to melt a highly attenuated frosty vapour in the air, and in this heated medium, the spires were situated.

In the month of July, the same year, I observed a perpendicular elongation of faintly reddish light from the setting sun to the clouds. Its breadth was equal to the sun's diameter.

The western sky was covered with clouds, except an opening round the sun 15° high, and 12° wide. The departing rays, as is usual in this country in summer, coloured the edges of the clouds, wherever they penetrated, a scarlet red; and from the unevenness of the surface, gave the illuminated parts the appearance of flame.

This elongation of light appeared to extend forward nearly to the observer. The place where I stood was on rising ground near the town; at about half a mile in front was the river Oupa. In the vapour rising from the river, and the dampy holm on this side of it, originates this elongation of the sun's figure.

ARTICLE IX.

ANALYSES OF BOOKS.

Memoirs of the Astronomical Society of London. Vol. I. London. 1822.

(*Concluded from* p. 153.)

The second of the three memoirs furnished by the Rev. William Pearson, is entitled, "On the Construction and Use of a Micrometrical Eye-piece of a Telescope." The rationale of the new contrivance is given, previously to explaining the application of the doubly-refracting power to some of the most delicate measurements in practical astronomy; but this preliminary disquisition, which is extended to a considerable length, not admitting of intelligible abridgment, nor indeed bearing very materially upon the subject, we shall proceed to give Dr. Pearson's account of the application of this power. "In measuring the diameter of a small body of sensible dimensions, the sliding tube containing the prism must be steadily and gradually moved by the finger and thumb, backwards or forwards, until after adjustment for good vision, the two images of the object come exactly into contact, edge to edge: in this situation the distance indicated, will be the argument for entering the table of powers; and the power there seen by inspection, will be the proper argument for entering the table of measures, which will give at sight the apparent diameter, in seconds and parts of a second, without further correction. When the angular distance between two stars, satellites, or other luminous points, is required to be measured, there will be two pairs of images formed, and these may fall in any direction with respect to each other; but turning the moveable tube with the prism round more or less will bring the four luminous images into one straight line; in which position, if the second and third images coincide exactly, the measure will at once be correct; but if not, the distance between the lenses must be varied until this coincidence takes place. Should the prism used be found to have too great or too small an angle at any of the distances marked in the scale, it must be changed for another having a more suitable angle, and must be adjusted as before directed. In all cases where one of two contiguous stars is much smaller in appearance than the other, and is yet visible through the prism, the small one will be lost by super-position on the larger, and must, therefore, be made to pass over its centre, by a slow motion given by rotation of the tube, when an estimate may be made of the exactness of the central transit; or otherwise, the four visible images may be formed into an exact square, when it will appear whether or not the bounding sides of the figure are equal to each other; and if they are, the proper distance will be indicated in that position." These directions are illustrated by a tabular account of the actual application of the micrometrical eye-piece to various celestial measurements; and the memoir concludes with extensive tables of powers and measures, of which it is not possible to give an intelligible abridgement.

Of the sixth memoir, "On the Construction of a New Position-Micrometer, depending on the Doubly-refractive Power of Rock Crystal," a very short notice will be sufficient. Before proceeding to describe the addition made to the former instrument, a method is mentioned by which Dr. Pearson varies the constant angle of a prismatic solid, by the juxta-position of a second solid of double refraction; a method which, says he, "to me is new, but which probably may be known to those philosophers, who have studied more minutely the laws of the polarization of light." By what arrangement the eye-piece micrometer with double images is converted into a position-micrometer, the following extract will render sufficiently obvious. "When a crystal of the micrometer was applied before the eye-piece of a transit instrument, all the spider's lines, as was expected, were seen double; as was also a star or other luminous point placed at a distance. But turning the prism round a little, soon brought all the images of the vertical lines into contact with the lines themselves, and the coincidence was perfect as to breadth, but not as to length of the lines in question: the image of the star in the mean time revolved round the star itself without coming into contact. Likewise when two stars, in the same field of view, are examined through a doubly refracting prism, a line connecting either star and its own image will be truly vertical, when the image of the vertical line is coincident with the ine itself, which may always be made so by turning round the prism. While the image of the vertical line was separated from the line itself to its greatest distance, by turning the prism, the image of the star circulated round the star the space of an exact quadrant. In this situation the horizontal line and its image coincided as to breadth, but not as to length, just as the vertical line and its image had done before: and separating them to their greatest distance, brought the vertical line and its image again into a state of coincidence; while the image of the star moved through another quadrant. The same appearances took place in the quadrantal point of the other semicircle. This experiment led to an immediate conclusion, that if a vernier connected with the revolving prismatic solid, were made to travel along a graduated circle, until a pair of stars and their images are all seen arranged in one straight line, it would indicate, in that position, the angle that this line makes with the vertical or horizontal line, accordingly as the graduations might be figured on the limb; provided that the zero of the circle has been previously adjusted to the vernier, while one of the vertical or horizontal lines had its respective image coincident therewith." The instrument was completely adapted to its purpose by substituting for the thread of a spider's web, which was liable to be injured by turning the tube or cleaning the lens, a fine line made by drawing a diamond diametrically across the plain face of the lens.

The next memoir contains " Observations on the best Mode of examining the Double or Compound Stars; together with a Catalogue of those whose Places have been identified, by James South, Esq. FRS. FLS. &c." In consequence of Sir William Herschell having employed in the examination of double stars, instruments of powers much greater than fixed instruments generally possess, a method has been given by him for finding a double star, not only equal, but indeed as he asserts, superior to having its right ascension and declination given. At the time when this opinion was expressed, and for several years afterwards, the highest power belonging to any of the fixed instruments did not exceed 80: they were, therefore, quite inadequate to this particular species of astronomical research. Now, however, it appears that telescopes admitting of magnifying powers equal to 500 or 600 are attached to fixed instruments; and in all cases where compound stars can be resolved by such powers, Sir William Herschell, it may be inferred notwithstanding all that he has said apparently to the contrary, would fully dmit their convenience and sufficiency. The difference, thereore, betwixt the author and this celebrated astronomer is a mere hadow; and did not demand in its delineation any of those xpressions of diffidence, which deference to an authority so levated, if actually opposed, would inevitably call forth. "The eculiar fitness of fixed instruments," says Mr. South, " may be substantiated by their superior steadiness; by the unerring certainty with which they may be directed to the wished-for star; by the opportunity they afford us of examining any star at its most advantageous situation; by the uniformity in the appearance of the compound stars, which they present to the eye and position of the observer, thereby materially assisting him in subsequent observations; and, lastly, by the facility which they afford to the dispatch of business."

A letter from Professor Gauss, dated Observatory, Gottingen, and addressed to Mr. Herschell, the Foreign Secretary, gives an account of "the new Meridian Circle at Gottingen," constructed by the celebrated artist Reichenbach. It is adapted at once for a transit, and for the measurement of altitudes, and possesses (in common with the most perfect meridian telescopes) all the adjustments requisite for their purpose. The telescope is five Paris feet in focal length, and four Paris inches in aperture. The four eye-pieces magnify respectively 68, 86, 120, and 170 times. The cross wires consist of seven vertical and two horizontal spider-threads. The intervals between the former are each traversed by an equatorial star in 14″. The horizontal ones are only 7″·6 asunder. The axis, 33 Paris inches in length, carries on one side two concentric circles whose outer surfaces (or those furthest from the telescope) lie nearly in one plane. The exterior circle (which being fastened on the axis, revolves with the telescope), bears the divisions which are to every three minutes. The inner, or *alidade* circle, would turn freely about the axis, were it not for a clamp fastened on the pillar. This allows it only a small delicate motion for the purpose of adjusting the level fastened upon it. On this alidade circle are the four indices, each 45° from the vertical line, with their verniers; which subdivide the principal division into 90 parts each, and consequently from 2″ to 2″, and yet smaller parts admit of estimation. The diameter of the circle where the reading-off takes place is 35 Paris inches. That both circles, without being in actual contact, are yet separated by an interval scarce perceptible, and that in consequence, the microscopes for reading-off are purposely set somewhat obliquely, the surface of the dividing circle standing out a little, although but extremely little beyond that of the alidade circle, are adjustments which this instrument possesses in common with others by the same artist. Such, with the addition of a few unimportant particulars, is the account of this valuable instrument. Of its great correctness striking instances are furnished by the observations of M. Gauss; but we think it right not to extend this article further than to insert the following notice, by the Foreign Secretary: "A point which has occupied the attention of astronomers for some years, though it involves only a few seconds, is yet of the highest importance, both in reference to the art of astronomical observation, and on account of the numerous astronomical elements, whose exact determination depends on it; I mean the minute difference in the declinations of stars, the obliquity of the ecliptic, and the altitude of the pole, which appear in their determination by different, though very excellent instruments. There is no doubt these differences arise from the action of gravity on the different parts of each instrument, though hitherto the mode of this action has not been clearly pointed out, nor is it possible to pronounce decidedly which instrument has afforded the right and which the wrong result. We know, in fact, very little of the extent to which the yielding of the metals may go; and it seems too hazardous to deny the possibility of this cause exercising a notable influence on the divisions, and in consequence on the observations in any instrument, whatever be its construction, without grounding such denial on sufficient proof. In our meridian circle the great artist has done every thing to obviate the flexure of the telescope by a well adapted system of counterpoises: still a doubt may remain, whether all the flexure be done away with by that means, or rendered quite insensible; and the only direct means of ascertaining the point seems to be, the combination of immediate observations of a heavenly body with those of its image reflected in an artificial horizon. Such observations must of course be frequently repeated to clear up a point of so much delicacy. M. Gauss has already entered on this inquiry by observing the pole-star in a reflecting surface of water. It is, perhaps, the most striking proof of the astonishing optical power of the telescope, that the superior culmination may be very well observed in this manner even in the day time. The result of the first complete observation of this kind was as follows:

May 13, 1820.—Zenith distance of the north star, free from refraction, but including the error of culmination.

Inferior culmination	Direct	319°	50′	20·73″
	Reflected	220	5	3·94
Superior ditto	Direct	323	8	41·51
	Reflected	216	46	44·31

Hence we deduce the true zenith distance.

Inferior culmination	40°	7′	21·60″
Superior ditto	36	49	1·40

And hence (the change of declination in 12 hours being — 0″·1) the latitude of the place of the water vessel is 51° 31′ 48″·45, and that of the centre of the circle 51° 31′ 48″·40. This being nearly a mean between the two above given, it is rendered very probable that the effect of its weight on the observations with this instrument are either quite insensible or at least extremely small."

Such observations as Mr. Baily was able to make on the Solar Eclipse which took place on the 7th of Sept. 1820, with those which were communicated to him by accurate observers, constitute the ninth memoir in this collection. Considering the number of good observers who must have witnessed this interesting phenomenon, the number and importance of the communications is not so great as Mr. Baily had reason to expect; more especially, we may be allowed to add, considering the very scientific and impressive manner in which astronomers had been awakened to the occurrence, by the extensive gratuitous circulation of a dissertation on that eclipse, printed for that very purpose by Mr. Baily: an example of disinterested exertion, at once honourable to the author and to science. According to Mr. Baily's observations made at Kentish Town, north latitude 51° 33′ 34″, and west longitude 35″·2 in time, the beginning was 0h 21′ 42″·4, the end 3h 13′ 41″·1 (mean time at the place), and the duration, therefore, 2h 51′ 58″·7. The apparent diameter of the moon, and the distance of the borders of the sun and moon, were measured with great accuracy; and the steps taken for measuring, as well as for correction, are very clearly displayed. A barometer, with a thermometer within and without, were also noticed during the progress of the eclipse, but without observing any alteration in either of them. The diminution of light he states to have been very trifling; by no means so great as in the eclipse which occurred Nov. 1816, although only ·78 of the sun's disc was then obscured, and ·87 on the occasion now adverted to. Venus was seen, however, by thousands of spectators with the naked eye, and Mars was visible to many. We pass over the observations of Mr. Dollond, Mr. Groombridge, and Dr. Pearson, which do not convey any new or curious information on the subject, to insert those of Mr. Wiseman, of Norwich, which are both new and curious. After stating that, according to Mr. Wiseman, the eclipse began at Norwich (north latitude 52° 38′, east long. 5′ 10″ in time from Greenwich), at 0h 28′ 45″, and ended at 3h 21′ 40″, Mr. Baily thus continues the narrative: "This gentleman has also sent me the result of some experiments on the power of the burning lens on different substances, during the time of the eclipse. Having procured a piece of pasteboard, he affixed thereto four equal pieces of different coloured cloths; viz. black, blue, yellow, and red; and placed them successively in the focus of a burning lens on the day preceding the eclipse. The following are the periods at which they respectively took fire: viz.

Black in seven seconds.
Blue in seven seconds.
Red in eight seconds.
Yellow in sixteen seconds.

He also, on the same day, submitted the bulb of a thermometer (which then stood at 66°) to the focus of the lens; and in 1½ minute it rose to 94°, and probably would have risen higher, had he not been apprehensive that the glass would have been broken by the heat. These experiments were made at about two o'clock in the afternoon, in order that they might correspond with the time of the eclipse at its greatest obscuration. On the following day, about half an hour after the commencement of the eclipse, he applied the cloths in succession to the focus of the lens, and found the periods at which they respectively took fire to be as follow; viz.

Black in twenty seconds.
Blue in twenty seconds.
Red in sixteen seconds.
Yellow in forty seconds.

At about half an hour before the end of the eclipse, he again submitted them to the focus of the lens, and found their periods of ignition to be as under; viz.

Black in seventeen seconds.
Blue in eighteen seconds.
Red in fourteen seconds.
Yellow in twenty-four seconds.

But during the time of the greatest obscuration, he could not produce any effect upon them whatever. The thermometer at the commencement of the eclipse was at 66°, and by two o'clock had fallen to 61¾°. This was about the middle of the eclipse; and Mr. Wiseman assures me that at this time he held the bulb in the focus of the burning lens for upwards of four minutes, but without producing any sensible effect. At a quarter past two, he repeated the same experiment, and with the same result, although the sun was free from clouds. At the termination of the eclipse, the thermometer rose to 64°. Mr. Wiseman also states that he fitted up a prism in a darkened room, and that he made several observations on the coloured rays which were thrown on a screen of white paper. He says, that during the continuance of the eclipse, the yellow and blue rays were generally increased in brilliancy, whilst the red became exceedingly faint, and did not occupy more than half their usual breadth. As I am not aware that any experiments of a similar kind were made during this eclipse, and as the results are somewhat singular, although anticipated by Mr. Wiseman, I have thought it right to state them here, in order that the attention of the public may be excited thereto in any future eclipse." Mr. Baily received some communications from the Continent, which tend to confirm the observations made by former astronomers, on this singular and rare phenomenon. The formation of the annulus is mentioned by Mr. Albert, at Frankfort on the Maine, by Prof. Stark, at Augsburg, who represents its duration to have been 5′ 47″·5, by Prof. Schwerd, at Spire, who describes a bright spot at the point of one of the horns, six seconds before the annulus was formed, and by M. Nicolai, at the Observatory of the Grand Duke of Baden, at Manheim, whose account deserves to be transcribed. "The actual formation of the annulus was very remarkable; for about a second before it occurred, the fine curve of the moon's disc, then immediately in contact with the edge of the sun, appeared broken into several parts; and in a moment these parts flowed together like drops of water or quicksilver placed near each other. At the dissolution of the annulus, a similar appearance presented itself; for the delicate thread of light then formed by the annulus, instead of being broken in one place only, was, in an instant, divided in several places at once. The thermometer (reduced to Fahrenheit's scale) was at the commencement of the eclipse at 66¼°, and fell towards the middle to 63°, but afterwards rose again to 66¼°."

Prof. Moll, of Utrecht, in a memoir entitled, like the preceding, "On the Solar Eclipse which took place on Sept. 7, 1820," has transmitted an account of numerous observations made at Amsterdam, Groningen, and Middelburg. Prof. Van Swinden's statement respecting the formation of the annulus is exceedingly interesting, but without the aid of his figures would not be well understood: besides, we suspect that the venerable author of the *Positiones Physicæ* has experienced at the time, perhaps through his enthusiasm in the cause, certain optical delusions. Mr. Grave, also of Amsterdam, adds likewise some remarks as to the appearance of the eclipse. He made use of an English reflector made by Mann. The formation of the annulus appeared to Mr. Grave the most beautiful phenomenon which he ever beheld, and he has delineated it with considerable effect.

Of the remaining memoirs composing this valuable volume, we cannot convey any important account, in the abridged form to which we are confined: we regret the less, therefore, that the space still remaining allows only the repetition of their titles: viz. On the Comet discovered in the Constellation Pegasus in 1821, by M. Nicollet, of Paris. On the Comet discovered in the Constellation Pegasus in 1821; and on the luminous appearance observed on the dark side of the Moon on Feb. 5, 1821, by Dr. Olbers, of Bremen. On a luminous appearance seen on the dark part of the Moon in May, 1821, by the Rev. M. Ward. On the Occultations of Fixed Stars by the Moon; on the Repeating Circle; on the Perturbations of the new Planets; and Observations on the late Comet and the Planet Vesta, by Prof. Littrow, of Vienna. On the Places of 145 new Double Stars, by Sir William Herschell, President of the Astronomical Society. Universal Tables for the reduction of the Fixed Stars, by S. Groombridge, Esq.; and, lastly (17th memoir), Observations of the Solar Eclipse which took place on Sept. 7, 1820; communicated in a letter from M. Piazzi to the Foreign Secretary.

ARTICLE X.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Abbé Haüy.*

The following account of the accident which occasioned the death of this eminent philosopher, has been transmitted by a medical friend at Paris to the Editor, accompanied with a copy of Baron Cuvier's discourse at his funeral, the original of which our readers will prefer to any translation:—

On the afternoon of the 14th of May, while alone in his cabinet, the Abbé Haüy fell down, I rather imagine in consequence of a slip, for I cannot find that he suffered any loss of sensibility, nor did he subsequently exhibit any symptom of cerebral affection, which could warrant the idea of a fit.

After some little time, he managed to call his attendants to his assistance. Some days elapsed before the exact nature of the injury which he had received was ascertained, the pain which he experienced from it, added to that which he before acutely suffered from a nephritic complaint, rendering a very minute examination difficult. After some time, however, a fracture of the neck of the *os femoris* was discovered. Fortunately for the Abbé, he was attended by Almand, surgeon to the Hospice Salpetriere; and as this gentleman does not, like many of his countrymen, entertain vain hopes of reunion in fractures of this kind, the good old man was spared the fatigue of a useless and distressing apparatus.

Notwithstanding the diminution in his strength and appetite, the Abbé continued to cherish the prospect of recovery till almost the last; and in conversation with the few scientific friends who were permitted to see him, he exhibited full proof of the unabated vigour of his recollection and reasoning powers. A few days before his death, which occurred about nine o'clock on the morning of the 1st of June, it was discovered that a collection of matter had formed, after the evacuation of which, his decline became more rapid than it had previously been. The extreme heat of the weather, probably had some effect in accelerating it.

In consequence of the Abbé Haüy's being Canon of Notre Dame, custom required a considerable service to be celebrated in the cathedral on the occasion of his funeral, but the circumstance of its happening on the day on which that building was occupied by the Chamber of Deputies, in the performance of the usual ceremony on the election of new members, prevented this service from being performed there, and a dispensation was obtained to go through it in the Abbe's parish church. My engagements prevented me from being present on this occasion, but the ceremony must have been one of considerable length, for though the corpse left the garden between 10 and 11, it did not reach Père la Chaise till nearly three o'clock.

Here too I was nearly prevented from attending, for it happened that on that very day, the students of Law and Medicine were desirous of paying anniversary honours to a young man of the name of Allemand, who, two years before, had been killed by the gens d'armes. With a view to thwart them, the police placed troops to stop the different avenues to the burying-ground. By some of these I was forbidden entrance, but I was more successful in a second attempt, when I walked by the side of the corps. Several of the company were less fortunate, and were absolutely refused admittance. Gay-Lussac, the President of the Institute this year, attended with several of his fellow-members; and the Baron Cuvier read the speech, of which I have sent a copy.

While this was being read, some of the Abbé's brother professors, whose years indicated that they must ere long follow him, were melted into tears. Most of the company, according to custom, sprinkled *holy* water over the grave before leaving it.

A small body of the veteran troop employed as the guard at the garden, attended the funeral, and while standing at the ground, fired twice in platoons, and on retiring they singly discharged their pieces into the grave.

The new edition of the Abbe's Mineralogy will very shortly be published; he had, as he told me, very nearly brought it to a conclusion himself. He laboured at it very closely, and was as anxious about his success and credit as any young author could be.

FUNERAILLES DE M. L'ABBE HAUY.

Le 3 *Juin*, 1822, *ont eu lieu les funérailles de M. l'Abbé* (*René-Just*) *Haüy, Membre de l'Académie Royale des Sciences. Arrivé au lieu de la sépulture, M. le Baron Cuvier, Secrétaire Perpétuel de l'Académie Royale des Sciences, et Directeur du Muséum d'Histoire Naturelle, a prononcé, au nom des deux établissemens le discours suivant.*

MESSIEURS.—Par quelle fatalité la mort semble-t-elle depuis quelque temps se plaire à redoubler ses coups?

En peu de jours nous avons accompagné, vers ses tristes et derniers demeures, les Hallé, les Richelieu, les Sicard, les Vanspaendonck.

Ni les talents, ni les grandeurs, ni les services rendus à l'humanité n'ont pu a doucir ses arrêts.

Elle frappe aujourd'hui le génie et la vertu; elle nous enlève à la fois le plus parfait modèle du scrutateur de la nature, et celui du sage, heureux de la jouissance de la vérité, de ce bonheur sur lequel ne peuvent rien ni les révolutions ni les caprices du sort.

Au milieu d'occupations obscures et laborieuses, une idée vient sourire à M. Haüy; une seule mais lumineuse et féconde. Dès lors il ne cesse de la suivre, son temps, les facultés de son esprit, il lui consacre tout. Pour elle il étudie la minéralogie, la géométrie, la physique; il semble vouloir devenir un homme tout nouveau!

Mais aussi quelle magnifique récompense accordée à ses efforts?

Il dévoile la sécrete architecture de ces productions mystérieuses où la matière inanimée paraissait offrir les premiers mouvements de la vie; où il semblait qu'elle prît des formes si constantes et si précises, par des principes analogues à celles de l'organisation.

Il sépare, il mesure par la pensée les matérieux invisibles dont se forment ces étonnants édifices; il les soumet à des lois invariables; il prévoit par le calcul les résultats de leurs assemblages; et parmi des

milliers de ces calculs, aucun ne se trouve en défaut. Depuis ce cube de sel que chaque jour nous voyons naître sous nos yeux, jusqu'à ces saphirs et ces rubis que des cavernes obscures cachaient en vain à notre luxe et à notre avarice, tout obéit aux mêmes règles ; et parmi les innombrables métamorphoses que subissent tant de substances, il n'en est aucune qui ne soit consignée d'avance dans les formules de M. Haüy.

Comme un de nos plus illustres confrères a dit avec raison, qu'il n'y aura plus un autre Newton, parce qu'il n'y a pas un second système du monde, un peut aussi, dans une sphère plus restrainte, dire qu'il n'y aura point un autre Haüy, parce qu'il n'y a pas une deuxième structure des cristeaux.

Semblables encore en cela à celles de Newton, les découvertes de M. Haüy, loin de perdre de leur généralité avec le tems en gagnent sans cesse, et l'on dira et qu'il en a été de son génie comme de ses découvertes. Loin que l'âge ôtât quelque chose au mérite de ses travaux c'étaient toujours les derniers qui étaient les plus parfaits, et les personnes-qui ont vu l'ouvrage auquel il travaillait dans ses derniers moments, nous assurent qu'il sera encore le plus admirable de tout.

Quelle douce existence que celle qui se dévoue ainsi toute entière au culte d'une vérité grande et certaine ; d'une vérité autour de laquelle se groupent chaque jour de nouveaux faisceaux de vérités subordonnées. Combien un tel spectacle éclipse aux yeux de l'homme digne d'en jouir, ce que le monde peut lui offrir de plus brillant, et qui jamais l'apprécia mieux que M. Haüy. Ces objets même qu'il étudiait sans cesse, ces pierreries qu'une aveugle fureur va chercher si loin, au prix de tant de fatigues et quelquefois au prix de tant de sang, ce qu'elles ont de précieux pour le vulgaire etait précisément ce qui lui demeurait étranger. Un nouvel angle dans le plus commun des cristaux l'aurait intéressé plus que les trésors des deux Indes. Ces joyaux si chers à la vanité, ces diamants dont les rois eux-mêmes sont fiers de parer leur couronne, passaient journellement dans sons humble réduit sans l'émouvoir au milieu de sa simplicité !

Que dis-je ? tous le fracas du monde extérieur ne le laissait pas moins impassible. Il n'a été ébranlé ni par les menaces des hommes farouches qui en voulurent un instant à sa vie, ni par les hommages qu'à d'autres époques, des hommes en pouvoir se firent un honneur de lui rendre. Dans tous les temps un jeune homme studieux, un élève capable de saisir ses idées, avait plus de droits sur lui.

Lors même que sa santé ne lui permettait pas de se rendre dans son auditoire, il aimait à s'entourir de cette jeunesse, à lui prodiguer ses conseils, à lui distribuer ces productions curieuses de la nature, que l'estime de tous les hommes instruits faisait affluer de tous côtés dans sa collection.

Mais ce que ces nombreux élèves trouvaient encore pres de lui, de supérieur à ses dons et même à ses leçons, c'etait son example ; c'était l'aspect de cette douceur inaltérable à chaque instant récompensée par le tendre dévouement de sa famille ; celui de cette piété simple et tolérante, mais que les spéculations les plus savantes ne détournaient cependant d'aucun de ses exercises ; le spectacle enfin de cette vie si pleine, si calme, si considérée, dont ce que le monde et la science ont de plus illustre s'est efforcée d'adoucir les dernières souffrances.

Qu'ils bénissent donc la memoire d'un si bon maître ; qu'ils n'ou-

blient jamais le modèle qu'il leur laisse, et que, près de son tombeau, en se promettant de l'imiter, ils réjouissent encore son ombre.

Et nous mêmes, mes chers collègues, au milieu des larmes que nous arrache une perte si douleureuse, cherchons quelques consolations dans ces souvenirs; disons nous bien: quel homme jouir ici bas d'un bonheur plus constant? quel homme fut jamais plus certain d'un bonheur éternel?

II. *Volcano in Iceland.*

According to the last but imperfect news from Iceland, the volcano in Eyafields-jokull had remained quiet until the 26th of June, when a new eruption of ashes took place, which seems to have done more harm than the former. It is reported that the foot of the mountain had burst, and that a current of lava had begun to flow. The inhabitants of the nearest villages have been obliged to leave their houses. On the north part of the island frequent earthquakes have been felt, but they were not violent, and have done little damage.

III. *Jeffersonite.*

A new mineral, to which the above name is given, has been discovered at the Franklin Iron Works, by MM. Vanuxem and Keating, about six miles to the north-east of the town of Sparta, in Sussex County, New Jersey.

The following description of this mineral is given by the last-named gentleman:

"This mineral has hitherto been found in lamellar masses, the largest of which does not exceed a pigeon's egg, imbedded in Franklinite and Garnet.

"It presents three distinct cleavages, two of which are considerably easier than the third. These cleavages lead us for a primitive form to a rhomboidal prism, with a base slightly inclined. The angles of the prism are 106° and 74°, those of the inclination of the base are 94° 45′ and 85° 15′. There is another face, which makes with the vertical face of the prism, angles of 110° and 70°. I have likewise seen, in one instance, cleavages parallel to a rhomboidal prism of 116° and 64°. I have also obtained cleavages under an angle of about 99° 45′ and 80° 15′. I have not been able to trace the connexion between these and the former, but I am inclined to think, that they result from the combination of the two prisms just mentioned. I had hoped, as some of the cleavages have a tolerable degree of lustre, to have been enabled to determine the angles by the reflecting goniometer, but all my attempts to that effect have proved unsuccessful. I have not been able to obtain a reflection from any one face.

"The hardness of this mineral is intermediate between that of fluor spar and apatite. It is very readily scratched by pyroxene (malacolite).

"Its specific gravity varies from 3·51 to 3·55. I have in one instance obtained it as high as 3·64, but I suspect the mineral to have been mixed with Franklinite.

"Its colour is dark olive-green, passing into brown.

"It is slightly translucent upon the edges.

"Its lustre is slight, but semi-metallic upon the faces of cleavage; in the transverse fracture, it is resinous.

"The fracture is lamellar when in the direction of cleavage, otherwise it is uneven.

"When scratched with a knife, the streak is greyish.

"The colour of the powder is a light-green.

"Before the blowpipe, it melts readily into a dark-coloured globule.

"It displays no electric signs, either naturally or by heat or friction.

"It is not magnetic, either in the common way, or by the ingenious method of double magnetism, which we owe to Abbé Haüy.

"The acids do not act upon it when cold. When digested a long time with boiling nitro-muriatic acid, about 1-10th is dissolved. The residue is of a lighter colour."

The analysis was performed by Mr. Keating, who found it to consist of

Silica	56·0
Lime	15·1
Protoxide of manganese	13·5
Peroxide of iron	10·0
Oxide of zinc	1·0
Alumina	2·0
Loss by calcination	1·0
Loss	1·4
	100·0

The following remarks are added by Mr. Keating:

"The jeffersonite presents some points of resemblance with the pyroxene of Haüy, but still it can be well distinguished from it. Its cleavages are essentially different from those of the pyroxene, but appear to approach some of the faces of crystals of substances which have been united to this species; for instance, the angles in the diopside (mussite and alalite), fassaite, and in the *pyroxene analogique*, come very near some of the angles of cleavage obtained in the jeffersonite. I at first indulged the idea that these cleavages might be considered as cleavages parallel to the faces of secondary crystals of pyroxene, but upon reflection I am fully convinced that this is not the case; for the angles which we have measured cannot be deduced from the others by a strict mathematical calculation, and though they may approximate, they are not the same. Besides, no analogy can warrant us in admitting, that the regular cleavages of one substance can disappear entirely, and be replaced by cleavages parallel to secondary crystals. On the contrary, wherever minerals have been found presenting different orders of cleavage, the first or those parallel to the primitive form were always predominant. Thus in carbonate of lime, it is not uncommon to meet the cleavage parallel to the *equiaxe*, but I believe in every instance the *primitive* is predominant. In a rarer and more interesting instance, that of fluor spar, Prof. Mohs has described, and I have seen in his possession in Freyberg, specimens of the Saxon fluor which cleaved in the direction of the cube and the dodecahedron, but the octahedral cleavage was very distinct. Before we change our opinion on this point, we must change all our ideas of cleavage, and of its high importance in the determination of minerals.

"In the hardness there is also a remarkable difference, the pyroxene being decidedly harder. The specific gravity is likewise different: the highest specific gravity of pyroxene recorded by Haüy is that of a large crystal from Vesuvius, which gave 3·3578. The highest specific gravity indicated by Mohs is 3·5, while that of the jeffersonite has, in every instance which I have seen, exceeded this limit.

"The chemical analysis offers another important difference, in the absence of magnesia, which appears to be essential to pyroxene.

"For these and other reasons, I conceive that there can be no doubt as to the necessity of considering this mineral as a distinct species. I am inclined to believe that a closer study of the diopside and fassaite, and of the *pyroxene analogique*, might lead to their separation from the pyroxene and union with the jeffersonite. This is a subject which appears to me fraught with interest, but upon which I am not able to offer any thing but conjectures, as my specimens of these minerals are not as good as would be necessary to enable me to decide this point. I shall close these remarks merely by observing that a similar opinion is, I believe, entertained by Mr. Vanuxem."

IV. *Instrument for measuring the Compression of Water.*

Prof. Oersted has used a very simple instrument for measuring the compression of water. He fills a cylinder of glass with water which has been deprived of its air, the cylinder has on its upper end an air-tight cover of brass, through which a screw passes with a small piston of brass on its lower end, which presses on the water. In the cylinder is a thermometer tube filled with the same water as the cylinder, and having on its upper open end a small column of mercury which, the tube being very narrow, remains there without sinking into the bulb. Suppose now the water being pressed in the cylinder by screwing down the piston, this pressure will act equally powerful on the open end of the tube, as on the outside of the bulb, so that the pressure being equal on the interior and on the exterior side of the glass bulb, neither expansion nor contraction of its walls can take place, the state of the mercury above the water in the glass tube will, therefore, immediately indicate the compression. Professor Oersted had previously ascertained the capacity of the bulb and of the tube by weighing the mercury which they were able to hold. The pressure exerted by the screw on the water was measured by another tube filled with air, likewise inclosed in the cylinder. Thus he obtained the result, that the compressibility of water diminishes very quickly with the increase of pressure, and that the mean compressibility at a pressure of 3 to 4 atmospheres is $\frac{45\cdot0}{1000000}$ for each atmosphere, which agrees pretty well with the experiments of Canton.

V. *Tutenag.*

This substance has lately been analyzed by Dr. Fyfe. The following is the Doctor's account of the specimen he examined:

"Dr. Howison, of Lanarkshire, was so fortunate, when in China, as to procure a basin and ewer of Chinese or white copper, a part of which he sent me for analysis. From the experiments I have performed on it, I find the composition to be different from what is stated by the above-named chemists, its component parts being copper, zinc, nickel, and iron; the last of which, however, is but in small quantity.

"The basin in the possession of Dr. Howison is of a whitish colour, approaching to that of silver, and is very sonorous. When held in one hand, and struck with the fingers of the other, the sound is distinctly heard at the distance of an English mile. It is also highly polished, and does not seem to be easily tarnished. The piece that was sent me I found was malleable at a natural temperature, and at a red heat; but when heated to whiteness, it was quite brittle, breaking with the slightest blow of a hammer. By great caution, it was rolled into thin plates, and was drawn into wire, of about the thickness of a fine needle. When fused in contact with the atmospheric air, it oxidated, and burned with a whitish flame, in the same way as zinc does. Its specific gravity at 50° was 8·432.

" Five grains of it were subjected to analysis, with the view of ascertaining the proportion of its ingredients; the result was,

		Or in the 100 parts,
Copper........	2·02	40·4
Zinc..........	1·27	25·4
Nickel........	1·58	31·6
Iron..........	0·13	2·6
	5·00	100·00

" The method which is practised in preparing white copper is not known in this country, though it seems to be the general opinion that it is procured by the reduction of an ore, containing the ingredients of which it is composed. In a letter I received from Dr. Howison, he mentions, that Dr. Dinwiddie, who accompanied Lord Macartney to China, showed him, when at Calcutta, several specimens of the ore from which he was told the white copper was procured, and which he obtained at Pekin. The basin, in the possession of Dr. Howison, cost in China about one-fourth of its weight in silver; and the exportation of utensils of this alloy is prohibited. These circumstances also render probable the opinion, that the white copper is obtained by the reduction of a metallic ore, for in China, labour is cheap, and the metals composing it are said to be found in great abundance.—(Edin. Phil. Jour.)

ARTICLE XI.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

Mr. P. W. Watson, of Hull, is preparing a work, to be entitled " Dendrologia Britannica: containing an Account of the Trees and Shrubs that will live in the open Air of Britain the whole Year;" and to be illustrated with coloured Plates from living Plants.

Mr. Worsdale, sen. of Lincoln, has ready for the press, " Celestial Philosophy, or Genethliacal Astronomy," containing the whole Art of calculating Nativities, and a great number of Genitures. To be published in 25 Numbers, 8vo.

Mr. Wood is preparing a complete Illustration of his Index Testaceologicus, in which he will give an accurate Figure of every Shell.

Mr. T. Coar has in the press, the Aphorisms of Hippocrates, with Translations into Latin and English.

Anatomical and Physiological Commentaries, by Herbert Mayo, Surgeon, and *Lecturer on* Anatomy. 8vo. To be published in *Numbers.*

JUST PUBLISHED.

Observations (from Experience) on the Aid obtained in various Diseases, particularly those incidental to Tropical Climates, by the External Application of Nitromuriatic Acid in a Bath. With several Cases, wherein it has been used by the Author with great Utility. To which is added the present most approved Mode of mixing the Acids and preparing the Bath. By Phineas Coyne, MRCS. London, &c. 8s. 6d. Boards.

An Introduction to the Study of Fossil Organic Remains, especially of those found in the British Strata; intended to aid the Student in his Inquiries respecting the Nature of Fossils, and their Connexion with the Formation of the Earth. By James Parkinson, FRCS. MGS. WSE. and Cæsarean Society, Moscow. With Plates. Post octavo. 12s.

Geological Essays; comprising a View of the Order of the Strata, the Coal Fields and Minerals, of the District of the River Avon; an Introduction concerning Primitive, and the Flood-washed Earth; Refutation of Errors, and Notes from the Best Authors. By Joseph Sutcliffe, AM. Author of a Grammar of the English Language. 8vo. 4s.

The Study of Medicine: comprising its Physiology, Pathology, and Practice. By John Mason Good, MD. FRS. Member of the Royal College of Physicians, London, &c. 4 large Vols. 8vo. 3l. 4s.

Article XII.

NEW PATENTS.

D. Gardner, Edmund-place, Aldersgate-street, for a stay, particularly applicable to supporting the body under spinal weakness, and correcting deformity of shape.—June 18.

J. Wass, Ashover, Derbyshire, millwright, for an improvement, which prevents the ill effects to vegetation and animal life that has hitherto been occasioned by noxious fumes and particles that arise from smelting or calcining lead ore, &c.—June 15.

M. I. Brunel, Chelsea, engineer, for improvements on steam-engines.—June 26.

T. Gauntlett, Bath, surgeons'-instrument-maker; for improvements on vapour-baths, by which the heat is better regulated, and the baths rendered more portable.—June 26.

W. Brunton, Birmingham, engineer, for improvements upon fire-grates, and the means of introducing coal thereon.—June 26.

L. B. Rabant, Skinner-street, Snow-hill, Gent. for an improved apparatus for the preparation of coffee or tea.—June 26.

T. Postans, Charles-street, St. James's, Gent. and W. Jeakes, Great Russell-street, Bloomsbury, ironmonger, for an improvement on cooking apparatus.—June 26.

G. Smart, Pedlar's Acre, Lambeth, civil engineer, for an improvement in the manufacture of chains, which he denominates mathematical chains.—July 4.

J. Smith, Sheffield, book-keeper, for an improvement of, or in, the steam-engine boiler.—July 4.

ARTICLE XIII.

METEOROLOGICAL TABLE.

1822.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.	Daniell's hyg. at noon.
7th Mon.								
July 1	N	30·13	30·05	72	46	—		
2	S W	30·13	30·02	72	48	—		
3	N W	30·04	30·02	78	53	—		
4	Var.	30·04	29·85	78	56	57	16	
5	Var.	29·95	29·80	72	46	—	70	
6	N	30·17	29·95	71	44	—		
7	N W	30·23	30·17	76	52	—		
8	N W	30·23	30·14	77	58	—		
9	N W	30·14	29·94	74	58	50	—	
10	W	29·94	29·91	75	56	—	10	
11	S W	29·91	29·51	75	53	—	30	
12	W	29·94	29·51	71	50	—		
13	N W	30·10	29·94	70	44	57		
14	N W	30·10	30·04	84	45	—		
15	S E	30·04	29·87	74	52	—		
16	N	29·87	29·80	75	57	—	50	
17	S W	29·82	29·80	75	52	46		
18	S E	29·82	29·60	79	60	—	23	
19	S W	29·62	29·60	78	56	—		
20	S	29·66	29·62	75	56	—		
21	S	29·76	29·66	76	60	56	10	
22	N W	29·86	29·76	76	58	—		
23	S W	29·85	29·70	72	60	—	25	
24	S W	29·75	29·70	74	59	—	03	
25	S W	29·84	29·75	73	51	57		
26	S W	29·87	29·84	74	48	—		
27	S W	29·87	29·64	78	58	—	41	
28	S W	29·64	29·61	72	56	—	05	
29	W	29·64	29·62	74	53	—	14	
30	N W	29·77	29·64	67	46	58		
31	S W	29·90	29·77	75	49	10	26	
		30·23	29·51	84	44	3·91	3·23	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Seventh Month.—1. Cloudy. 2—4. Fine. 5. A thunder storm about two, a. m.: a very heavy shower about half-past ten: the rain continued till about two, p. m. with very frequent thunder at a distance. 6, 7. Fine. 8. Cloudy. 9, 10. Fine. 11. Showery. 12—15. Fine. 16. Fine: night rainy. 17. Fine. 18. Fine: night rainy, with thunder. 19, 20. Fine. 21. Showery. 22. Fine. 23. Showery. 24—28. Fine. 29. Showery: some thunder. 30. Fine. 31. Showery.

RESULTS.

Winds: N, 3; SE, 2; S, 2; SW, 11; W, 3; NW, 8; Var. 2.

Barometer:	Mean height	
	For the month	29·862 inches.
	For the lunar period, ending the 11th	30·058
	For 15 days, ending the 9th (moon south)	30·067
	For 13 days, ending the 22d (moon north)	29·810
Thermometer:	Mean height	
	For the month	63·741°
	For the lunar period	63·431
	For 31 days, the sun in Cancer	64·500
Evaporation		3·91 in.
Rain		3·23

Laboratory, Stratford, Eighth Month, 23, 1822. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

OCTOBER, 1822.

Article I.

liminary Account of a new Class of Compounds of Sulphur. W. C. Zeise, Doctor of Philosophy, and Professor of Che-stry in the University at Copenhagen.*

a certain quantity of sulphuret of carbon is poured into a ion of potash, soda, or ammonia, in alcohol, either pure, or ining a small portion of water, a neutral liquid is obtained, h the sulphuret of carbon by itself never shows any properties: the reason is, that this sulphuret combines other substances, and forms a new acid which is capable of alizing the alkalies. If potash has been used, the new formed nay be obtained either by great refrigeration, or by evapo- , or it may be precipitated by sulphuric ether. The salt ins neither a trace of carbonic acid, nor sulphuretted gen, but the acid in this compound is of a peculiar nature, ontains carbon, sulphur, and hydrogen; it is the same with ct to sulphuret of carbon that the hydrocyanic acid is respect to cyanogen. I have called its compounds with hydrocarbosulphates. Some of the most remarkable pro- s of the hydrocarbosulphate of potash are the following: s salt crystallizes in long needles, or in a fibrous mass, com- of similar crystals. It has a peculiar smell even after g been dried under the air pump; its taste is a little like r, peculiar, however, and very strong; it remains dry in en air, and is very soluble in water; the aqueous solution is colourless, and completely neutral. The salt is readily soluble in alcohol, but dissolves difficultly in sulphuric ether. Both solutions are also colourless and neutral. No gas is expelled by strong acids; but when sulphuric or muriatic is added to a concentrated aqueous solution of the salt, an oil-like liquid is separated, which has a rather yellow colour, and a peculiar strong smell.

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* Extract of a paper read at the Royal Society at Copenhagen, May 17, 1822.

1. The solution produces with the soluble salts of lead a white precipitate.
2. With the nitrate of barytes, muriate of barytes, muriate of lime, no precipitate at all.
3. With soluble salts of copper, a fine yellow precipitate.
4. With nitrate of deutoxide of mercury, sublimate and prussiate of mercury, a white precipitate.
5. With nitrate of deutoxide of tin, a yellowish precipitate.
6. With sulphate of zinc, a white precipitate, a little inclining to green.
7. With nitrate of silver, nitrate of protoxide of mercury, muriate of protoxide of tin, if the solutions are diluted, a yellowish, if concentrated, a brownish precipitate.

All these precipitates, except those of No. 7, keep their colour unchanged under the liquid, and in open air. None of them produce gas when treated with strong acids, but some give the above-mentioned oil-like substance. The hydrocarbosulphate of potash may be heated to 140° Fahrenheit without undergoing any observable change. When heated in close glass vessels, it melts, effervesces, assumes a fine light-red colour, and gives out, 1. an oil-like liquid of a yellow colour, and a peculiar strong and very penetrating smell; it does not show any acid properties when tried with tests, and does not produce a black precipitate with salts of lead; 2. Carbonic acid; 3. Another gas, which is either a mixture of sulphuretted hydrogen and a peculiar gas, or this latter alone: it is remarkable for its strong smell of onion. The red substance remains unaltered when cooled, but when the temperature is increased to a red heat, it melts, effervesces, and becomes brownish-black, and after some time ceasing to effervesce, the substance melts quietly. On cooling, the substance separates into two distinct portions; the lower and larger mass is crystalline and greyish-black; the upper is uncrystalline and black. During this change, besides gas, a great quantity of an oil-like liquid is given out. If the black substance is heated again, it melts, produces no oil-like liquid, and but little gas, even if the temperature is raised to a strong red heat. Afterwards it has no crystalline appearance.

The red mass above-mentioned dissolves easily in water without the least turbidness. In the beginning, the solution has a red colour, which, however, is soon changed into brown. In alcohol, it is difficultly dissolved; the solution is yellowish-brown. The red mass is alkaline, when tried by tests. It becomes moist in the open air, but not much so. The aqueous solution, when mixed with a solution of nitrate or acetate of lead, throws down a blood-red precipitate, which slowly changes into black, exactly as Berzelius has described. He obtained it when mixing nitrate of lead with an aqueous solution of potash that had remained during three weeks with a surplus of sulphuret of carbon.* Even when the solution of the red melted substance has assumed the brown colour, it produces a red precipitate with salts of lead; but, which is curious, the precipitate which has been thrown down by a brown solution blackens much quicker than that from a red solution. When a piece of the melted red substance before it has attracted moisture is thrown into a solution of nitrate of lead, the precipitate keeps its colour during several days, principally if, after it has been washed with water, alcohol is poured on it, and it is then quickly dried. When sulphuric, muriatic, or acetic acid is poured on the red mass, a violent effervescence takes place, and the smell of sulphuretted hydrogen is perceived, together with some other smell; an oil-like liquid separates, but neither pure sulphur nor pure carbon. By being exposed for about 30 hours to the atmosphere, it does not lose much of its colour, but in longer time it becomes yellow.

The greyish-black crystalline mass deliquesces quickly when exposed to the air, and when tried with tests, it shows free alkali. The aqueous solution is brownish-black without depositing any observable powder, but the intensity of its colour is so great that it only becomes transparent on being much diluted. When an acid is poured in it, it gives out a great quantity of gas which seems to be pure sulphuretted hydrogen; a great quantity of carbon is precipitated, and but little sulphur. None of the oil-like liquid appears. When a solution of it is exposed to the air, it remains more than 24 hours without getting turbid; afterwards carbon is precipitated, at last in great quantity, and the liquid becomes colourless.

The black mass deliquesces very readily, and contains free alkali. It dissolves easily in water, while a great quantity of carbon in flakes is deposed; the solution is first greenish-yellow, and at last, when sulphur has precipitated, it is colourless. When an acid is mixed with a recently prepared solution, sulphuretted hydrogen is expelled, and a great quantity of sulphur is thrown down. I consider the crystalline black mass prepared by heating the red substance to be a compound of potassium and a kind of sulphuret of carbon; the black uncrystalline mass is a mixture of sulphuret of potassium and carbon.

When the hydrocarbosulphate of potash is exposed to the heat of the flame of a candle, it inflames and burns, throwing about a great number of brilliant sparks. Two periods may be distinctly observed in this combustion: during the first, it melts into a reddish-brown substance, which, when again inflamed, burns and throws the sparks still more violently around. The red mass itself is likewise combustible, and exhibits immediately violent combustion with white sparks. If the salt be thrown on red-hot glass, or charcoal, it is quickly consumed. I apprehend the cause of this phenomenon to be, that a compound of potassium and sulphur is suddenly formed, while carbon and a gas are separated, and that these small particles of carbon when thrown about produce the white burning sparks. I find this theory supported by the phenomena which take place when the salt is decomposed by heat in close vessels.

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* *Afhandlingar i Fysik*, Kemie og Mineralogie, 5 Deel, p. 266, &c.

The hydrocarbosulphate of soda crystallizes more difficultly, and in a form quite different from that of the salt of potash; it deliquesces in moist air, and is not separated from its solution in alcohol by means of sulphuric ether; with acids and salts of metals, it exhibits the same phenomena as the hydrocarbosulphate of potash.

Hydrocarbosulphate of lime is obtained by mixing a solution of muriate of lime in alcohol, with a solution of the hydrocarbosulphate of potash; muriate of potash is precipitated, and the hydrocarbosulphate of lime remains pretty pure, dissolved in the alcohol; for though the muriate of potash is soluble in alcohol, the salt of lime almost entirely prevents its solution.

I consider the compounds that are separated when the hydrocarbosulphate of potash is mixed with solutions of certain metals to consist of the metal and a kind of sulphuret of carbon without oxygen. Zinc, however, may make an exception. The carbosulphuret of copper has a fine lively yellow colour, the carbosulphuret of lead and the carbosulphuret of mercury are white; the first, however, has a foliated, shining, crystalline appearance; the second is granular. The carbosulphuret of copper is prepared by pouring an aqueous solution of the hydrocarbosulphate of potash into a solution of sulphate or nitrate of copper. The precipitate, when washed with water, is pure. The carbosulphuret of lead is prepared in the same way from nitrate of lead, and the carbosulphuret of mercury from corrosive sublimate, or prussiate of mercury. They are all insoluble in water, but the carbosulphuret of lead and of mercury at least are soluble in alcohol. The carbosulphuret of mercury is soluble in a concentrated solution of the hydrocarbosulphate of potash, and it seems to form with it a saline compound. Strong acids act very slowly on these compounds, and by themselves they may be exposed to the heat of boiling water without being decomposed. When heated in a glass tube, the carbosulphuret of copper and that of lead produce at a certain temperature a mist in the vessel, which is condensed into a yellow liquid, with a smell like onions, and the exact appearance of oil; afterwards it melts, gives out gas with violent effervescence, and in considerable quantity; then passing through different shades, it assumes a brownish-black colour, and the heat being increased, it becomes completely black, and whilst exhibiting the phenomena of combustion. The gas does not contain any carbonic acid, and seems to be the new gaseous compound of carbon and sulphur.

The substance remaining when the carbosulphuret has thus been exposed to a red heat is a mixture of a metallic sulphuret and carbon. I have reasons for considering the brown mass before it has been exposed to a red heat, as a new metallic carbosulphuret, either with another kind of carbosulphuret than that which exists in the precipitated substance, or with the same, but in less quantity. The carbosulphuret of mercury shows the same phenomena, except that at a pretty high temperature, a substance is sublimed which resembles cinnabar, and a black scaly mass remains which is charcoal.

The precipitate obtained from sulphate of zinc and the salt of potash consists, when dry, of small greenish-white heavy grains, soluble both in water and alcohol; however, in a much greater quantity in the latter. The alcoholic solution furnishes, when evaporated, white opaque globular masses. When newly precipitated, it is easily decomposed by sulphuric and muriatic acid, and furnishes an oil-like liquid, similar to that which is procured from the hydrocarbosulphate of lime by a similar process. When heated, this compound of zinc is changed into a mass of a pretty intense green colour, which, when an acid is poured on it, gives out with violent effervescence a gaseous compound of sulphuretted hydrogen, and another peculiar gas. Paper, previously moistened with a solution of lead, becomes, when exposed to these gases, on different places spotted blood-red and black; the first being the prevailing colour.

The hydrocarbosulphuric acid is insoluble in water, or at least nearly so; its specific gravity is greater than 1·0. It may easily be procured by pouring a mixture of one part of sulphuric acid and three-fourths of a part of water on the hydrocarbosulphate of potash, and adding, in a few seconds, a great quantity of water. The acid soon collects on the bottom of the vessel as a completely transparent almost colourless oil; it must be quickly washed with water until it ceases to give a precipitate with a solution of muriate of barytes. The acid may almost entirely be freed from water by decantation.

When litmus paper is brought into this acid, it becomes instantly red; if the paper remains exposed to the air, it becomes partially yellow and white. The smell of this acid is completely different from that of sulphuret of carbon; its taste is acid, and strongly astringent. Exposed to the air, it is soon covered with a yellowish-white coating. It is easily inflamed, and when burned, gives out the smell of sulphurous acid. It is decomposed by heat.

It is easily dissolved by a solution of potash in water, which is thereby neutralized, and the liquid thus prepared has all the properties of an aqueous solution of that salt which has been made from sulphuret of carbon, alcohol, and potash. When water is present, this acid expels carbonic acid from carbonate of potash, carbonate of ammonia, and carbonate of barytes.

When black oxide of copper, red oxide of mercury, or oxide of lead, is thrown into this acid, there is instantly formed a yellow carbosulphuret of copper, white carbosulphuret of mercury, and carbosulphuret of lead. The same substances are formed when a solution of muriate or sulphate of deutoxide of copper, a solution of sublimate or of nitrate of lead is mixed with this acid. When I poured a small quantity of water on hydrocarbosulphuric acid just enough to cover it, and put a small quantity of iodine into the acid, decomposition took place, and after having added a fresh quantity of water, and shaken the mixture, an oil-like liquid separated on the bottom of the glass, which seemed to have all the properties of sulphuret of carbon. The liquid which covered it was weak hydriodic acid, which threw down the solution of sublimate of a beautiful red colour, the nitrate of lead of an equally fine yellow colour, and the nitrate of silver of a whitish colour. When a sufficient quantity of iodine is added to an aqueous solution of hydrocarbosulphate of potash, an oil-like liquid is soon separated on the bottom, which likewise seems to be a sulphuret of carbon, and the solution contains hydriodate of potash.

The following remarks may serve as a conclusion:

When a concentrated solution of potash, soda, or ammonia, is added to sulphuret of carbon in a small flask, a white coagulated mass is formed; when, after the flask being well closed and shaken, it is kept quiet for a moment, gas makes its escape with great violence, if the stopper is removed. The same effect takes place when the sulphuret of carbon is mixed with a small quantity of alcohol. The yellowish liquid out of which the salt of potash crystallizes rather slowly contains not a trace of sulphuretted hydrogen, and has in general the properties of a solution of hydrocarbosulphuret, probably mixed with some sulphur, and a peculiar compound of carbon and hydrogen, which certainly is the cause of the colour of the liquid. When the hydrocarbosulphate of potash is thrown down from its solution by ether, the remaining etherial liquid has the same properties as have been just enumerated as belonging to the liquid remaining after the crystallisation of the salt.

When I, some time ago, tried the effects of olefiant gas on chloride of sulphur, a substance was obtained, the smell of which strongly resembled that of the onion. Mercury, through which this air was passed, had the same smell in a great degree, and kept it for several days even in the open air. The same is the case when mercury is used for the above-mentioned decomposition.

If these two substances, with the same onion-like smell, though produced in quite a different way, are really the same, I am inclined to consider the action of chlorine such as that this body combines with *all* the hydrogen of the olefiant gas, allowing the sulphur and carbon to form a compound; or that it combines only with a certain quantity of hydrogen, while the rest, together with the carbon, combines with sulphur. I obtained the same onion-like smell a short time ago by passing chlorine through a solution of sulphuret of potash in alcohol. In this case, and when chlorine was passed through sulphuret of potash, another interesting decomposition seemed likewise to take place, which, however, I must make the subject of a peculiar set of experiments.

ARTICLE II.

Reply to Mr. Winch. By G. Young, Esq. and J. Bird, Esq.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Whitby, Aug.* 31, 1822.

NOT being regular readers of your *Annals*, we did not know till last week that a letter appeared in your number for May, from the pen of N. J. Winch, Esq. animadverting on some passages in our Geological Survey of the Yorkshire Coast, and complaining that we have travelled out of our road for the purpose of writing strictures on his geological essays.

Having no idea that we had done any injustice to Mr. Winch, or to other writers, whose mistakes we had ventured to correct, we are rather surprised to notice his complaints. In describing the strata of the Yorkshire coast, and their connexion with those of the county of Durham, we could not conceive that we were going out of our way by pointing out the errors of those who had gone before us; on the contrary, we felt it to be a duty which we owed to truth and to the public. This duty we have endeavoured to discharge without any hostile feelings towards our fellow-labourers in the cause of science; and that our remarks on Mr. Winch's statements did not originate in any such feelngs, might be inferred from our acknowledging an instance of his politeness, and from our referring our readers to his paper on the Geology of Durham and Northumberland for an account of the magnesian limestone, and the strata that succeed it.—(See our Geol. Survey, p. 166, 170.)

On what ground Mr. Winch could consider our note respectng the organic remains in the limestone at Hartlepool as a reflection on his accuracy, we cannot tell. The most attentive observers will sometimes overlook interesting phenomena. We have probably made similar mistakes or omissions in our Survey; and if any literary friend shall point them out, we shall feel obliged, rather than offended. The shells in the Hartlepool limestone are not numerous; so that Mr. Winch might collect a hundred specimens containing none; and had our observations been confined to a single quarry, they might have escaped our notice as well as his. We found them, however, not only in quarries, but in the rocks along the shore, and in several stone walls, constructed with materials procured on the spot.

In the second instance quoted by Mr. W. there is no difference between him and us in our description of what is visible, but merely in our opinions respecting what is hypothetical; and he seems to have introduced the passage only for the sake of exposing a blunder of ours, in applying the term *basaltic* to the dyke near Cullercoats. But if he will read our words over again, he may perceive that the blunder is his own. We applied no such epithet to that dyke; for in comparing it with our basaltic dyke, we merely call it " a similar dyke or vein," as it intersects the strata in a similar way. Its materials form no part of the subject under discussion in the passage referred to, and do not affect our arguments.

Our remarks on the improper use of the term *coal basin* are produced by Mr. W. as another attack on him, though they are not aimed at any one author in particular; and though, like our observations on dykes, they oppose his statements only in matters of opinion. We do not call in question the accuracy of his sections, but the justness of the inferences which he would deduce from them. We maintain that coal strata are not more subject to undulations and depressions than other strata; and that where the coal strata form a basin, the strata above, and those below, must, to a certain extent, assume the same shape. Our opinion on this point coincides with that of Mr. Westgarth Forster, whose acquaintance with the coal and lead mines of Northumberland and Durham is at least not inferior to that of Mr. Winch. He remarks, in the new edition of his Treatise on a Section of the Strata, &c p. 13: " A seam, or bed, of coal is a real stratum, which is found to be quite as regular as any of the concomitant strata found in the coal-field, lying above or below the coal; or indeed as any other of the various strata which compose the external crust of our globe."

But the passage which has given most offence to Mr. Winch (and indeed the only one that can with any plausibility be considered as offensive) is that where we allege, that his geological " Observations on the Eastern Part of Yorkshire," are not the result of personal examination, but compiled from " scraps of information collected from others." To those who are unacquainted with this district, and consequently unable to decide on the merits of Mr. Winch's paper, our remark may appear severe;

but such as have carefully examined it, and compared it with Mr. W.'s description, will think with us, that whatever more we might have said in the way of reprehension, we could not well say less. Of the existence of that paper, as Mr. W. might have seen from the note itself, we had no knowledge till within a few days of the time when the note was printed. We were aware, that in 1814 and 1815, Mr. W. collected some information from Mr. Bird and others, concerning the strata of this part of Yorkshire; but knowing, from his own letters, that he was then very imperfectly acquainted with the subject, and never hearing that he had subsequently made any excursions into the district, we could not suppose that he had attempted to write a geological description of it; and, on meeting with the document, we could not but regret that such a paper had found its way into the Transactions of a Society so respectable. Mr. W. it seems, has, at some remote periods, travelled through the district on business, and taken notes; but however frequent his journeys, and however copious his notes, his own paper, independent of what we know otherwise, warrants us to say, that he has not given the district that kind of examination which is necessary for writing a geological description of it. We have, perhaps, traversed the counties of Durham and Northumberland more frequently than he has done our district; yet we are far from supposing ourselves qualified to give, from our own observations, a tolerable account of their geology. We were detracting much less from Mr. W.'s fame, when we intimated that his paper was a compilation from very scanty and incorrect materials, than we should have done, had we supposed it possible for Mr. W. after a personal examination, to write a description so confused, so defective, and so inaccurate. We have paid him at least this compliment, that we could not rate his talents so low. Part of the blunders in his paper might indeed be placed to the score of inadvertency or defective memory; such as his describing the red sandstone of the vale of the Tees as " devoid of mica," with which, in most parts where we examined it, it greatly abounds; his stating that the ironstone of the coal measures is the material employed at the alum works in manufacturing Roman cement, whereas that material is obtained in the alum shale, and does not consist of ironstone, but of lias nodules, the nodules containing much iron or pyrites being rejected as unfit for the purpose; and his describing the oolite as cropping out at Filey Head, and stating that " with this material, York Minster and other edifices in the neighbourhood, are constructed." These, and other minor mistakes, might possibly be committed by one who had examined the district. But how could any gentleman who had spent even but a day or two in acquiring only a tolerable idea of the disposition of the strata, have produced such a mass of error and confusion as is found in Mr. W.'s description of our hills? He speaks of Danby Beacon as part of " the northern escarpment of the Cleveland chain," though it is one of the most southerly parts of that chain. He describes the Cleveland chain as "succeeded at the vale of the Esk by the oolite limestome ridge;" thus *totally overlooking the most lofty and extensive chain of our alum-hills,* intervening between the vale of the Esk and the oolite hills. This is not a mere inadvertence, or slip of the pen, for he repeats the error presently after by representing the oolite hills as "a *lower* range of hills, rising to view about three miles south of Robin Hood's Bay;" the very position of our *highest* range of hills, which he has passed over, and which forms the most prominent feature in the Eastern Moorlands, comprising Stoupe Brow Hill, Loosehone Moor, Burton Head, Cranimoor, &c. some of which rise about 1400 feet above the level of the sea. Of this vast chain, Mr. W. gives no account, passing immediately from the Cleveland chain, on the north side of the Esk, to the oolite hills; which last he also characterises as *round* topped, though they are notoriously *flat* topped. After an omission so egregious, it is scarcely necessary to notice other gross mistakes in Mr. W.'s account of the strata; such as his confounding the blue limestone of the vale of Pickering with that of Thirkleby; his making our alum shale to pass from Arncliff south to Cowsby (mis-printed Cosley), and thence to Thirkleby; whereas it is well known that it reaches but a mile or two beyond Osmotherley; and his making it "terminate on the coast below Scalby" (mis-printed Scalesby), though it is seven or eight miles short of the Scalby shore. Mr. W.'s list of organic remains is meagre in the extreme, but not more so than we might expect, knowing from his letters, that so late as April, 1815, he had only seen some specimens of the Whitby petrifactions, without knowing to what portions of the strata they belonged; and *did not then know whether the oolite* (which swarms with shells) *contained any organic remains or not.* Part of his list seems to have been copied from some Scarborough document without altering even the local phrases; for some petrifactions are said to occur "on the sands," while we are not told what sands are meant, nor whether they occur there in the strata, or are washed out of the alluvial cliffs.

We shall only add (for we have, perhaps, said more than enough), that we are not the only authors who have complained of Mr. W.'s inaccuracies. Mr. W. Forster, in the work above quoted, Pref. p. 9, states that Mr. Winch, in describing the lead measures, has "made many mistakes, for want of a correct local knowledge of the country."

It is painful for us thus to expose the faults of a brother geologist, but Mr. W. by publicly accusing us, has compelled us to perform this unpleasant task in our own vindication.

We are, Sir, your most obedient servants,

George Young.
John Bird.

ARTICLE III.

On the Finite Extent of the Atmosphere. By William Hyde Wollaston, MD, VPRS.*

THE passage of Venus very near the sun in superior conjunction in the month of May last, having presented an opportunity of examining whether any appearance of a solar atmosphere could be discerned, I am in hopes that the result of my endeavours, together with the views which induced me to undertake the inquiry, may be found deserving of a place in the Philosophical Transactions.

If we attempt to estimate the probable height to which the earth's atmosphere extends, no phenomenon caused by its refractive power in directions at which we can view it, or by reflection from vapours that are suspended in it, will enable us to decide this question.

From the law of its elasticity, which prevails within certain limits, we know the degrees of rarity corresponding to different elevations from the earth's surface; and if we admit that air has been rarefied so as to sustain only 1-100th of an inch barometrical pressure, and that this measure has afforded a true estimate of its rarity, we should infer from the law, that it extends to the height of 40 miles, with properties yet unimpaired by extreme rarefaction. Beyond this limit we are left to conjectures founded on the supposed divisibility of matter: and if this be infinite, so also must be the extent of our atmosphere. For if the density be throughout as the compressing force, then must a stratum of given thickness at every height be compressed by a superincumbent atmosphere, bearing a constant ratio to its own weight, whatever be its distance from the earth. But if air consist of any ultimate particles no longer divisible, then must expansion of the medium composed of them cease at that distance, where the force of gravity downwards upon a single particle is equal to the resistance arising from the repulsive force of the medium.

On the latter supposition of limited divisibility, the atmosphere which surrounds us will be conceived to be a medium of finite extent, and may be peculiar to our planet, since its properties would afford no ground to presume that similar matter exists in any other planet. But if we adopt the hypothesis of unlimited expansion, we must conceive the same kind of matter to pervade all space, where it would not be in equilibrio, unless the sun, the moon, and all the planets, possess their respective shares of it condensed around them, in degrees dependent on the force of their respective attractions, excepting in those instances where the tendency to accumulate may be counteracted by the interference of other kinds of matter, or of other powers of which we have no experience, and concerning which we cannot expect to reason correctly.

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* From the Philosophical Transactions, for 1822. Part I.

Now, though we have not the means of ascertaining the extent of our own atmosphere, those of other planetary bodies are nevertheless objects for astronomical investigation; and it may be deserving of consideration, whether, in any instance, a deficiency of such matter can be proved, and whether, from this source, any conclusive argument can be drawn in favour of ultimate atoms of matter in general. For, since the law of definite proportions discovered by chemists is the same for all kinds of matter, whether solid, or fluid, or elastic, if it can be ascertained that any one body consists of particles no longer divisible, we then can scarcely doubt that all other bodies are similarly constituted; and we may without hesitation conclude, that those equivalent quantities, which we have learned to appreciate by proportionate numbers, do really express the relative weights of elementary atoms, the ultimate objects of chemical research.

These reflections were originally suggested by hearing an opinion hazarded without due consideration, that the non-existence of perceptible atmosphere around the moon might be regarded as conclusive against the indefinite divisibility of matter. There was, however, an oversight in this inference, as the quantity of such matter, which the moon would retain around her, could not possibly be perceived by the utmost power of any instruments hitherto invented for astronomical purposes. For, since the density of an atmosphere of infinite divisibility at her surface would depend on the force of her gravitation at that point, it would not be greater than that of our atmosphere is where the earth's attraction is equal to that of the moon at her surface. At this height, which by a simple computation is about 5000 miles from the earth's surface, we obviously can have no perceptible atmosphere, and consequently should not expect to discern an atmosphere of similar rarity around the moon.

It is manifestly in the opposite direction that we are to look for information. We should examine first that body which has the greatest power, and see whether even there the non-appearance of those phenomena which might be expected from such an atmosphere, will warrant the inference that our own is confined to this one planet by the limit set to its divisibility.

By converse of the same rule which gives an estimate of extreme rarity at the moon's surface, we may form a conception at what distance round the sun refraction from such a cause should be perceived. If we calculate at what apparent distance from the body of the sun his force is equal to that of gravity at the surface of the earth, it is there that his power would be sufficient to accumulate (from an infinitely divisible medium filling n space) an atmosphere* fully equal in density to our own, and onsequently producing a refraction of more than one degree, in he passage of rays obliquely through it.

If the mass of the sun be considered as 330,000 times that of he earth, the distance at which his force is equal to gravity will e $\sqrt{330{,}000}$, or about 575 times the earth's radius; and if his adius be 111,5 times that of the earth, then this distance will e $\frac{575}{111{\cdot}5}$ or 5·15 times the sun's radius; and $15' \, 49'' \times 5{\cdot}15 =$ 1° 21′ 29″, will be the apparent distance from the sun's centre n the 23d of May, when the following observations were made.

What deduction should be allowed for the effect of heat, it may be time to consider when we have learned the amount of apparent refraction at some given distance; and we may then begin to conjecture, whether heat can counteract the increase of density that would occur in the approach of only 1-10th of a second towards his centre.†

As I had not any instrument in my possession that I considered properly adapted for the purpose, I requested the assistance of several astronomical friends in watching the progress of Venus to the sun for some days preceding superior conjunction, and in recovering sight of her afterwards. But neither the Astronomer Royal at Greenwich, nor Prof. Brinkley of Dublin, nor Mr. South, with the admirable instruments they possess, were able to make any observation within the time required, not being furnished with the peculiar means adapted to this inquiry.

Capt. Kater, however, who entered fully into my views, and engaged in the prosecution of them with all the ardour necessary for success, by using a reflecting telescope, was able to furnish me with a valuable set of observations, 3½ days preceding conjunction, which, together with those in which I had the good fortune to succeed at nearly an equal interval subsequent to the passage, afford data quite sufficient to show that no refraction is perceptible at the period of our observations; and these come far within the specific distance above estimated.

A selection from the series given to me by Capt. Kater is contained in the following table:

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* Such an atmosphere would, in fact, be of greater density on account of the far greater extent of the medium affected by the solar attraction, although of extreme rarity; but the addition derived from this source, may be disregarded in the present estimate, without prejudice to the argument, which will not be found to turn upon any minute difference.

† If we attempt to reason upon what would be the progressive condensation of such an atmosphere downwards towards the surface of the sun, we are soon stopped by the limit of our experience as to the degree of condensation of which the atmosphere is susceptible. If we could suppose the common law of condensation to extend as far as 46 miles in depth, the density corresponding to it would be about equal to that of quicksilver, from whence a refraction would occur exceeding all bounds of reasonable calculation. A space of 46 miles at the distance of the sun from us would subtend about one-tenth of a second.

				Diff. R. A.		Diff. calc. from N. Alm.	
	h.	m.	s.	m.	s.	m.	s.
May 18	2	40	25	4	25·6	. . .	
	21	30	50	3	43·1	. . .	
	23	27	58	3	38·8	. . .	
19	0	0	0	,	. .	3	37
				Diff.	Decl.		
May 18	2	44	33	45	56		
	23	19	40	40	57		
19	.	.	.	. .	.	40	36

It is evident that, in these observations, the differences between the observed and calculated places of the planet are not such as to indicate a refraction that can be relied on.

My own observations were very few in number, and not to be compared to the former in precision; but they are necessary to supply a deficiency when Capt. Kater was at a distance from his instruments, and could make no observation.

On the 26th, between XI. 20 and XI. 30 I had three comparative observations, the best of which gave me the passage of Venus 3^{m} 55^{s} after the sun. The mean of two others being 3^{m} 49^{s}. I consider the result as on the 25th, 23^{h} 24^{m}. Diff. R. A. 3^{m} 52^{s}.

The nearest second to be inferred from the Nautical Almanac for this time being 3^{m} 53^{s} after the sun, it is evident that no perceptible refraction occurred at this time.

From the observations of Capt. Kater, no retardation of the motion of Venus can be perceived in her progress toward the sun, as would occur from increasing refraction; and by comparison of her motion in the interval between his last observation and my own, with her change of place for the same interval given in the Nautical Almanac, there seems no ground whatever to suppose that her apparent position has been in the least affected by refraction through a solar atmosphere, although the distance at the time of Capt. Kater's last observation was but 65′ 50″ from the sun's centre, and at the time of my own only 53′ 15″.

Although these distances appear small, I find that Venus has been seen at a still less distance by Mons. Vidal of Montpellier in 1805.* On the 30th of May, he observed Venus 3^{m} 16^{s} after the sun, when their difference of declination was not more than 1′, so that her distance from the centre was about 46 minutes of space. Since his observations also accord with the calculated places of Venus, they might have superseded the necessity of fresh observations, if I had been duly aware of the inference to be drawn from them.

The same skilful observer has also recorded an observation of

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* Conn. des Tems. 1808.

Mercury on the 31st of March of the same year, when he was seen at about 65′ from the sun's centre.

If I were to describe the little telescope with which my observations were made, without taking due care to explain the precautions adopted, and the grounds of their efficacy, it might, perhaps, be scarcely credible, that with an object glass less than one inch in aperture, having a focal length of only seven inches, I could discern an object not to be seen by telescopes of four and five inches aperture. We know, however, that this small aperture is abundantly sufficient for viewing Venus at a distance from the sun; and, since the principal obstruction to seeing her nearer (when the atmosphere is clear), arises from the glare of false light upon the object-glass, the success of the observation depends entirely on having an effectual screen for the whole object-glass, which is obviously far more easy to accomplish in the smaller telescope.

Since the screen which I employed was about six feet distant from my object-glass, a similar protection for an aperture of five inches would have required to be at the distance of thirty feet, to obviate equally the interference of the sun's light at the same period; but this is a provision with which regular observatories are not furnished for the common purposes of astronomy.

As I hope at some future time to avail myself of a larger aperture for such observations, without the necessity of mounting a more distant screen, it may be desirable that I should suggest to others the means by which this may be effected, if they think the question of a solar atmosphere worthy of further investigation.

If an object-glass of four inches aperture be covered, so as to expose only a vertical slit of its surface one inch in width, the surface of glass to be so used is about five times as large as the circular aperture one inch in diameter, and yet will be as completely shaded by a vertical screen at any given distance: and an interval of only five feet might allow a star or planet to be seen within a degree of the sun's disc.

When the sun and planet have the same declination, the vertical position of the slit is manifestly the most advantageous that could be chosen on the meridian; but, for the purpose of seeing to the greatest advantage when the line of the centres is inclined to the horizon, it would be requisite to have the power of turning the slit and screen together at right angles to any line of direction of the centres.

The only fixed star sufficiently near to the ecliptic, and bright enough to give any prospect of its being seen near the sun, is Regulus, which passes between the 20th and 21st of August, but I have not yet had an opportunity of ascertaining within what distance from the sun this star can be discerned.

In the foregoing remarks, I have, perhaps, dwelt more upon the consideration of the solar atmosphere, than may seem necessary to those who have considered the common phenomena observable in the occultations of Jupiter's satellites by the body of the planet. Their approach, instead of being retarded by refraction, is regular, till they appear in actual contact; showing that there is not that extent of atmosphere which Jupiter should attract to himself from an infinitely divisible medium filling space.

Since the mass of Jupiter is full 309 times that of the earth, the distance at which his attraction is equal to gravity must be as $\sqrt{309}$, or about 17·6 times the earth's radius. And since his diameter is nearly eleven times greater than that of the earth, $\frac{17\cdot 6}{11} = 1\cdot 6$ times his own radius will be the distance from his centre, at which an atmosphere equal to our own should occasion a refraction exceeding one degree. To the fourth satellite this distance would subtend an angle of about 3° 37′, so that an increase of density to $3\frac{1}{4}$ times our common atmosphere would be more than sufficient to render the fourth satellite visible to us when behind the centre of the planet, and consequently to make it appear on both (or all) sides at the same time.

The space of about six miles in depth, within which this increase of density would take place, according to known laws of barometric pressure, would not subtend to our eye so much as 1-300th of a second, a quantity not to be regarded in an estimate, where so much latitude has been allowed for all imaginable sources of error.

Now though, with reference to the solar atmosphere, some degree of doubt may be entertained in consequence of the possible effects of heat which cannot be appreciated, it is evident that no error from this source can be apprehended in regard to Jupiter; and as this planet certainly has not its due share of an infinitely divisible atmosphere, the universal prevalence of such a medium cannot be maintained; while, on the contrary, all the phenomena accord entirely with the supposition that the earth's atmosphere is of finite extent, limited by the weight of ultimate atoms of definite magnitude no longer divisible by repulsion of their parts.

Article IV.

Analysis of Mica with only One Axis of Double Refraction.

By M. Rose, of Berlin.*

Among a great number of different kinds of mica which Dr. Seebeck had examined with respect to their action on light, only a single one was found, which might be properly considered as possessing only one axis of double refraction. It was met with at Berlin in a collection of Siberian minerals, without any notice of the place where it had been found; it is highly probable that it was from Siberia. The analysis of this singular mica gave

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* From Gilbert's Annals, vol. v, 1822.

Silica	42·01
Peroxide of iron	4·93
Alumina	16·05
Magnesia	25·97
Potash	7·55
Fluoric acid	0·68
Manganese	Trace
	97·19

The great loss in this analysis depends certainly upon the difficulty of separating magnesia and potash so as to ascertain their exact quantity. All methods that have been made use of afford only approximation, and I know none which would give an exact result. The quantity of potash was ascertained in the following manner: Thin leaves of mica were carefully placed into a crucible with nitrate of barytes in alternating layers, and heated red hot. The ignited mass was dissolved in muriatic acid, the silica was separated by evaporation, the barytes by sulphuric acid, alumina and oxide of iron by ammonia. The liquid was evaporated to dryness, and the residuum after having been heated long enough to volatilize all the sulphate, and muriate of ammonia was dissolved in water, and the solution was mixed with acetate of barytes. The liquid separated by filtration from the sulphate of barytes was evaporated, and the dry salt heated red-hot. Water poured on it dissolved the carbonate of potash, which, after evaporation, was heated red-hot, and weighed. For further comparison, it was saturated with muriatic acid, and its weight again ascertained. In order to find the other component parts of this mica, the analysis was repeated with carbonate of potash instead of nitrate of barytes. Silica was obtained by evaporation to dryness in the usual way. After having been heated, the particles adhered together; it was not, therefore, perfectly pure silica, which, when heated, forms an extremely fine powder.

The liquid which had been filtered from the silica was treated with ammonia, and the precipitate thus obtained boiled with caustic potash. The oxide of iron precipitated was dissolved in muriatic acid, the solution neutralized by ammonia, and precipitated by succinate of ammonia. The alumina was precipitated from its solution in caustic potash by muriatic acid, redissolved by an excess of it, and again precipitated by carbonate of ammonia. The liquid which had been filtered from the precipitate by ammonia, and that which had been filtered from the succinate of iron were concentrated together by evaporation, mixed with carbonate of potash in sufficient quantity so as to decompose all the salts of ammonia, and evaporated to dryness. The dry mass was dissolved in water, boiled, and the magnesia thus obtained separated by a filter. When, after having been heated to redness, it was redissolved in muriatic acid, some silica remained undissolved, which is almost always the case in the analysis of minerals containing silica. The manganese which the magnesia contained was so extremely small that it could not be separated.

The reason why the silica obtained by this method had agglutinated could only be that fluoric acid existed in the mica. The insoluble triple compound of silica, fluoric acid, and potash, must have remained with the pure silica, and muriatic acid could not completely decompose it. By heating it, it lost its fluoric acid, and the potash combining with the silica made it agglutinate. To find the fluoric acid, the analysis was repeated a third time, which I did in the same manner as Berzelius made use of in analyzing the topaz.

This mica remaining unchanged both in external appearance and in weight, when exposed to a heat in which other varieties of mica that I had analyzed, had lost water and fluoric acid, I was surprised to find fluoric acid likewise in this kind. In a paper published two years ago, I ascertained to be a property of mica which contains much fluoric acid, that it does not lose its metallic lustre and become dull, even in a very strong heat, whereas those which contain small traces of fluoric acid change, according to my former statement, their colour by ignition, but they keep their metallic lustre. This mica, however, contains more fluoric acid than mica from Utoe, which loses its lustre easily in a moderate heat; while the same heat has no effect upon this mica. It is, therefore, no certain proof of the presence of a greater quantity of fluoric acid, that certain kinds of mica become dull in a moderate temperature. It seems as if fluoric acid escapes more easily from mica containing water, by which its appearance becomes dull. When the heat is increased, mica with one axis of double refraction also loses its lustre, and about two per cent of its weight. In respect to the chemical composition, this mica differs materially from those three kinds which I formerly analyzed, and for which I gave a formula that answered for all three kinds which likewise agree in their action on light. But whether the potash really is in the form of trisilicate contained in the mineral I am scarcely able to determine, the quantity of potash being difficult to ascertain exactly, and this substance containing not much oxygen. This, however, is certain, that oxide of iron and alumina are present, in the form of silicates, in the mica with two axes of double refraction.

In the mica with one axis of double refraction which I analyzed, the

	Per cent. of oxygen.
Silica contains	21·13
Oxide of iron	1·51
Alumina	7·5
Magnesia	10·05
Potash	1·28
Fluoric acid	0·5

From which we may conclude, that the oxygen in all the bases together amounts to the quantity of oxygen in the silica, that the oxygen contained in the bases with three atoms of oxygen (peroxide of iron and alumina) together with that of the potash, are equal to that of the magnesia. It is, therefore, possible that this mica consists of the common mica with two axes of double refraction (or of silicates of bases with three atoms of oxygen, combined with silicates of potash, like the mica of the former three analyses), and of mica composed of silicates of bases with two atoms of oxygen, like magnesia, by which combination, the interesting effect of this mica upon light is probably produced.

The kinds of mica with two axes of double refraction differ likewise by the effect of acids upon them considerably from those with one axis. The former are altogether insoluble in the strongest acids, while the latter is acted upon by acids, though with difficulty.

M. Peschier, of Geneva, published a short time ago a paper in which he asserts, he has found in many different kinds of mica a considerable quantity of oxide of titanium. I have tried with the blowpipe all the different species of mica which he mentions, without having been able to find the least trace of that metal in any one of them, though the oxide of titanium may with the greatest ease be discovered by this instrument. M. Peschier heated the mica with nitrate of barytes, dissolved the heated mass in muriatic acid, supersaturated the solution with carbonate of ammonia, and obtained the oxide of titanium from the thus remaining liquid, after having passed it through a filter. It is, however, not possible to obtain oxide of titanium in this way, which, when dissolved in acids, is completely precipitated by carbonate of ammonia.

Article V.

Additional Remarks on Dr. Thomson's Paper on the Effect of Aqueous Vapour on the Specific Gravity of Gases. By Charles Sylvester, Esq.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Carnarvon, Aug.* 15, 1822.

In my letter to you which appeared in the *Annals* for July, relative to Dr. Thomson's paper on the subject of high pressure steam, and the influence of aqueous vapour on the specific gravity of gases, my remarks were principally directed to Dr. Thomson's observations upon high pressure steam. I also made some allusion to his formula for correcting the specific gravity of gases from the pressure of aqueous vapour. Having since had a conversation with Mr. Dalton, of Manchester, I have been induced to attend more minutely to that part of Dr. Thomson's paper, and find that his formula will only be correct when the experiment is made at 212° for aqueous vapour, or the boiling point of the substance from which the vapour is derived. At any temperature above or below that point, the formula given by Dr. Thomson will fail. My object in this communication is to give a formula which, I believe, will give the true result at all temperatures.

Let P = a column of mercury equal to the pressure of the atmosphere; f = a column of mercury which a volume of any condensible vapour will support, unmixed with any other elastic fluid, and at any given temperature; S = the specific gravity of the gas under examination; c = the specific gravity of the vapour under the pressure P; and R = the resulting specific gravity. Then if we suppose the gas and the vapour to be in separate equal volumes, and at the temperature of the boiling point of the liquid producing the vapour, these volumes after mixture will be $1 + 1 = 2$, the general expression being $1 + \frac{f}{P}$ which when $f = P$, will be $1 + 1 = 2$. From the expression $1 + \frac{f}{P} = v$ the resulting volume, we have the following equation to find R the resulting specific gravity $\left(1 + \frac{f}{P}\right) R = S + \frac{fc}{P}$, and $R = \frac{PS + fc}{P + f}$, when $f = P$. $R = \frac{S + c}{2}$, each of the volumes equal 1.

I am, yours truly,
C. Sylvester.

ARTICLE VI.

On Logarithmic and Circular Series. By Mr. James Adams.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Stonehouse, near Plymouth, Aug.* 27, 1822.

THE insertion of the following series, &c. in the *Annals of Philosophy*, when you have a convenient opportunity, will oblige

Your humble servant,
JAMES ADAMS.

From the nature of logarithms, we have

$\log. (1+u) = u - \frac{u^2}{2} + \frac{u^3}{3} - \frac{u^4}{4} + \frac{u^5}{5} -$ &c. and $\log. \left(1+\frac{1}{u}\right) = \frac{1}{u} - \frac{1}{2u^2} + \frac{1}{3u^3} - \frac{1}{4u^4} + \frac{1}{5u^5} -$ &c. By subtraction, $l\,(1+u) - l\left(1+\frac{1}{u}\right) = l\left(\frac{1+u}{1+\frac{1}{u}}\right) = l\,u = \left(u - \frac{1}{u}\right) - \frac{1}{2}\left(u^2 - \frac{1}{u^2}\right) + \frac{1}{3}\left(u^3 - \frac{1}{u^3}\right) -$ &c.............(1)

By differentiating equation (1), we have $1 = \left(1 + \frac{1}{u}\right) - \left(u^2 + \frac{1}{u^2}\right) + \left(u^3 + \frac{1}{u^3}\right) - \left(u^4 + \frac{1}{u^4}\right) +$ &c.

Then by De Moivre's theorem for multiple arcs, we have $\frac{1}{2} = \cos. u - \cos. 2u + \cos. 3u - \cos. 4u + \cos. 5u -$ &c.. (2)

By addition $l\,(1+u) + l\left(1+\frac{1}{u}\right) = l\,(1+u)\left(1+\frac{1}{u}\right) = l\,\frac{(1+u)^2}{u} = l\left(u + \frac{1}{u} + 2\right) = \log. 2\left[\frac{1}{2}\left(u + \frac{1}{u}\right) + 1\right] = \left(u + \frac{1}{u}\right) - \frac{1}{2}\left(u^2 + \frac{1}{u^2}\right) + \frac{1}{3}\left(u^3 + \frac{1}{u^3}\right) -$ &c.

From whence we have $\frac{l\,(1 + \cos. u) + l\,2}{2} = \cos. u - \frac{1}{2}\cos. 2u + \frac{1}{3}\cos. 3u - \frac{1}{4}\cos. 4u +$ &c. By differentiating this last equation, we have $\frac{\sin. u}{1 + \cos. u} = \tan. \frac{1}{2}u = 2\,(\sin. u - \sin. 2u + \sin. 3u - \sin. 4u +$ &c.)............................(3)

By differentiating equation (3), we get $\sec.^2 \frac{1}{2}u = 4\,(\cos. u - 2\cos. 2u + 3\cos. 3u - 4\cos. 4u +$ &c.) .... (4)

Perhaps the following simple method relative to multiple arcs may not be unacceptable to the young analyst. By division

$\frac{1}{1+u} = 1 - u + u^2 - u^3 + u^4 - u^5 +$ &c.; and $\frac{1}{u+1} = \frac{1}{u} - \frac{1}{u^2} + \frac{1}{u^3} - \frac{1}{u^4} + \frac{1}{u^5} -$ &c. Subtract the second of these equations from the first, and we get $0 = 1 - \left(u + \frac{1}{u}\right) + \left(u^2 + \frac{1}{u^2}\right) - \left(u^3 + \frac{1}{u^3}\right) +$ &c.; by transposition and dividing by 2, we have $\frac{1}{2} = \frac{u + \frac{1}{u}}{2} - \frac{u^2 + \frac{1}{u^2}}{2} + \frac{u^3 + \frac{1}{u^3}}{2} - \frac{u^4 + \frac{1}{u^4}}{2} +$ &c. Then by De Moivre's theorem, $\frac{1}{2} = \cos. u - \cos. 2\,u + \cos. 3\,u - \cos. 4\,u + \cos. 5\,u -$ &c. ……………………………(5)

The same as equation (2), but independent of logarithms or differentiation.

In like manner, $\frac{1}{1-u} = 1 + u + u^2 + u^3 + u^4 +$ &c. And $\frac{1}{-u+1} = -\frac{1}{u} - \frac{1}{u^2} - \frac{1}{u^3} - \frac{1}{u^4} -$ &c. Subtract the second of these equations from the first, transpose and divide by 2, we shall then have $\frac{1}{2} = -\left(\frac{u + \frac{1}{u}}{2} + \frac{u^2 + \frac{1}{u^2}}{2} + \frac{u^3 + \frac{1}{u^3}}{2} + \text{\&c.}\right)$

Therefore $\frac{1}{2} = -(\cos. u + \cos. 2\,u + \cos. 3\,u + \cos. 4\,u +$ &c.) ……………………………(6)

By taking the successive differential coefficients of equation (5), we have

$0 = -\sin. u + 2\ \sin. 2\,u - 3\ \sin. 3\,u + 4\ \sin. 4\,u -$ &c.
$0 = +\sin. u - 2^3 \sin. 2\,u + 3^3 \sin. 3\,u - 4^3 \sin. 4\,u +$ &c.
$0 = -\sin. u + 2^5 \sin. 2\,u - 3^5 \sin. 3\,u + 4^5 \sin. 4\,u -$ &c.
$0 = +\sin. u - 2^7 \sin. 2\,u + 3^7 \sin. 3\,u - 4^7 \sin. 4\,u +$ &c.

……………………………

$0 = \sin. u - 2^{2n-1} \sin. 2\,u + 3^{2n-1} \sin. 3\,u - 4^{2n-1} \sin. 4\,u +$ &c. ……………………………(7)

And

$0 = -\cos. u + 2^2 \cos. 2\,u - 3^2 \cos. 3\,u + 4^2 \cos. 4\,u -$ &c.
$0 = +\cos. u - 2^4 \cos. 2\,u + 3^4 \cos. 3\,u - 4^4 \cos. 4\,u +$ &c.
$0 = -\cos. u + 2^6 \cos. 2\,u - 3^6 \cos. 3\,u + 4^6 \cos. 4\,u -$ &c.
$0 = +\cos. u - 2^8 \cos. 2\,u + 3^8 \cos. 3\,u - 4^8 \cos. 4\,u +$ &c.

……………………………

$0 = \cos. u - 2^{2n} \cos. 2\,u + 3^{2n} \cos. 3\,u - 4^{2n} \cos. 4\,u +$ &c.….(8)

We shall have from equation (7), when $u = 90°$,

$$0 = -1 + 3 - 5 + 7 - 9 + \text{\&c.}$$
$$0 = +1 - 3^3 + 5^3 - 7^3 + 9^3 - \text{\&c.}$$
$$0 = -1 + 3^5 - 5^5 + 7^5 - 9^5 + \text{\&c.}$$
$$0 = +1 - 3^7 + 5^7 - 7^7 + 9^7 - \text{\&c.}$$

……………………………

$$0 = 1 - 3^{2n-1} + 5^{2n-1} - 7^{2n-1} + \&c. \ldots\ldots (9)$$

And from equation (8), when $u = 0°$.

$$\begin{aligned} 0 &= -1 + 2^2 - 3^2 + 4^2 - 5^2 + \&c. \\ 0 &= +1 - 2^4 + 3^4 - 4^4 + 5^4 - \&c. \\ 0 &= -1 + 2^6 - 3^6 + 4^6 - 5^6 + \&c. \\ 0 &= +1 - 2^8 + 3^8 - 4^8 + 5^8 - \&c. \\ &\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ 0 &= 1 - 2^{2n} + 3^{2n} - 4^{2n} + 5^{2n}. \ldots\ldots (10) \end{aligned}$$

ARTICLE VII.

Results of Observations on the extraordinary Depression of the Barometer, which took Place on the 25th of December, 1821. By Prof. Brandes, of Breslau.

(To the Editor of the *Annals of Philosophy*.)

SIR,

THE extraordinary depression of the mercury in the barometer which was observed in England, France, and Germany, on the 25th of Dec. 1821, has attracted the attention of natural philosophers in each of those countries; and I presume, therefore, that it will be found very interesting to know what was the state of the barometer, and at what place it was lowest, &c. For the purpose of deciding these questions, I have brought together all the observations which I have been able to procure; and I have been fortunate enough to obtain sufficient materials for giving a complete table of what has been observed respecting this subject on the Continent.

The barometer was lower at Dieppe and at Boulogne than it was in any other part of the Continent. It will be very interesting to have the observations made in England, and I hope that the observers there will have the goodness to publish in the journals, with the utmost accuracy, first, the time when the least elevation of the barometer was observed; secondly, that height itself; and, thirdly, the mean altitude.* We should then be able to ascertain at what place the barometer was lowest; and we should see whether it was found lower in England than on the coast of the channel.

I have met with some observations in the English journals which give the time and the height very accurately, but the greater number of them do not mention the time of the lowest state of the barometer; nor even do they inform us whether the instrument was observed with precision at the moment of its greatest descent, although this is necessary in order to obtain correct results.

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* The journals which we receive regularly are the *Annals of Philosophy*, the Philosophical Magazine, and the Edinburgh Philosophical Journal.

The observations of which I here communicate the results were all made on the Continent; I have not noticed those which have been made in England, because there are not sufficient details of them for the formation of a table of what has taken place there; I hope, however, that I shall be able to give the results of the English observations at another opportunity.

Observations for determining the Time at which the Barometer was lowest in different Parts of the Continent.

At La Chapelle, near Dieppe, the minimum was observed by M. Breanté at 3^{h} 30′ in the morning.

At Troyes, at 3^{h}.

At Viviers, in the south of France, by M. Flaugergues, at 3^{h}.

At Boulogne Sur Mer, by M. Gambant, at 5^{h} 9′.

At Triers, at 5^{h}.

At St. Gallen, in Swisserland, by M. Meyer, before 5^{h}.

At Regensburg, by M. Heinrich, at 7^{h}.

At Strasburg, by M. Herrenschneidern, at 7^{h} 30′.

At Middleburg and at Utrecht, at 9^{h} 30′.

At Padua, nearly at the same time.

At Prague, by M. Hallasohka, at 10^{h}.

At Schwelm, near Elberfeld, on the Lower Rhine, by M. Castringius, at 1^{h} at noon.

At Hanover and Gottingen, by MM. Luthmer and Harding, at 12^{h}.

At Gotha, Jena, Halle, and Leipsic, according to the observations of MM. Kries, Posselt, Winkler, and Schmiedel, at 12^{h} or 1^{h}.

At Altona, by M. Schumacher, at 2^{h} 38′.

At Breslau, and at several other places in Silesia, at 2^{h} or 3^{h} in the afternoon.

At Cracow, by M. Markiewioz, at 3^{h}.

At Apenrade and Fredericksvark in Denmark, at 5^{h} or 6^{h}.

At Dantzic, by M. Kleefeld, at 9^{h} or 10^{h} in the evening.

At Abo, in Finland, on the 26th of Dec. in the morning.

At Dorpat, in the same day, at noon.

At Petersburg, in the evening of the 26th, and in the morning of the following day.

We may conclude with sufficient certainty from the above statement, that if we imagine a line drawn through those places where the barometer was at the minimum at the same moment, that line passed, on the 25th of December,

At 3 in the morning, through Dieppe, Troyes, and Viviers.

Before 5, through Swisserland.

At 5, through Boulogne and Triers.

t 7, through Strasburg, Regensburg, and Padua.
t 9 or 10, through Middleburg, Utrecht, and Prague.
t 12 or 1 at noon, through Elberfeld, Hanover, Gotha, and
sic.
t 2 or 3, through Altona, Breslau, and Cracow.
t 5 or 6, through Denmark.
t 9 or 10, through Dantzic.
n the 26th of December, through Abo, in the morning;
pat, at noon; and Petersburg, in the evening.

ght of the Barometer at the Moment of its greatest Depression.

Time of the Lowest Descent.	Places.	Height of the barometer in English inches and parts.	Amount of depression taken in the centre; in ditto.
5, 1821.—At 3^h in the morning.	Dieppe.	27·47	1·95
	Troyes.	28·20	1·46
	Viviers.	28·63	1·17
Before 5^h.	St. Gallen.	26·51	1·21
	Zurich.	27·31	1·22
At 5^h in the morning.	Boulogne.	27·91	2·00
	Paris.	28·33	1·45
	Triers.	28·24	1·19
At 7^h in the morning.	Strasburg.	28·18	1·39
	Regensburg.	27·52	1·27
	Padua.	28·96	0·95
At 9^h or 10^h in the morning.	Middleburg.	28·05	1·88
	Zwanenburg.	28·05	1·79
	Nurenburg.	27·68	1·33
	Prague.	28·06	1·15
At 12^h or 1^h at noon.	Elberfeld.	27·66	1·51
	Minden.	28·33	1·51
	Hanover.	28·31	1·43
	Gottingen.	27·99	1·40
	Leipsic.	28·23	1·34
	Gotha.	27·55	1·29
At 3^h in the afternoon.	Altona.	28·31	1·51
	Silesia. Jauer.	28·24	1·15
	Waldenburg.	27·29	1·16
	Breslau.	28·42	1·21
	Leobschux.	27·92	1·11
	Cracow.	28·08	1·11
At 5^h or 6^h.	Apenrade.	28·31	
	Frederiksvark.	28·39	1·60
	Christiania.	28·60	1·60
At 9^h or 10^h.	Dantzic.	28·71	1·39
6, 1821.—In the morning.	Abo.	29·01	1·24
At noon.	Dorpat.	28·87	0·97
In the evening.	Petersburg.	29·31	0·59

[It is to be understood of course that the barometer at Dieppe and Boulogne was lower with respect to its *medium* elevation at those places than any where else on the Continent: the greatest *absolute* depression appears to have been at St. Gallen, where, on the 25th of December, the mercury stood at 26·51; its mean height there being only 27·71, as appears from the table.—*Ed.*]

Article VIII.

Observations upon the Pulvis Antimonialis of the London Pharmacopœia. By Richard Phillips, FRS. L. and E. &c.

In a work published two years ago by Dr. Elliotson, containing the results of his experience in regard to prussic acid, he has related several examples of the exhibition of large doses of the pulvis antimonialis with little or no sensible effect. The quantity commonly prescribed is a few grains; 10 are seldom ventured upon; he found however, that from 90 to 100 grains might be given every 24 hours with perfect safety, and scarely any sensible effect.

He was led to exhibit these large doses on reading a paper in the first volume of the Dublin Hospital Reports, by Dr. Cheyne, who states that James's Powder is highly efficacious in removing the apoplectic diathesis, if given in gradually increased doses till some effect takes place upon the stomach, bowels, or skin. In endeavouring to produce some sensible effect with the pulvis antimonialis of the London Pharmacopœia, Dr. Elliotson found himself obliged to augment the dose up to 80, 90, 100, grains, and even more. He extended his trials to headache, palsy, epilepsy, and other cases, attended, if not by the apoplectic diathesis, by fulness of blood in the head. I may remark by the way, that he was not aware of any single patient receiving benefit from the medicine.

As an illustration, I will copy one case that occurred in an out-patient of St. Thomas's Hospital.

Aug. 26, 1819.—George Berring, aged 23. Ill four years. Rather short; extremely strong built; plethoric; head particularly large at the back part. Complains of violent pain running from the forehead through the head. Has had anaphrodisia for a twelve month, though he was formerly in the opposite extreme. Venesection, cupping, blisters, have been used in vain.

Let him take pulv. antim. gr. v. three times a day for three days; then gr. x. three times a day for three days; and finally, gr. xv. three times a day.

Sept. 4.—No better. Let him take gr. xv. three times a day for three days; then gr. xx. three times a day.

Sept. 18.—No better: has had no medicine for seven days. Let him take gr. xxv. for three days; then gr. xxx. three times a day.

Sept. 25.—No better. Yesterday he took once gr. xxx; once gr. xl; and once gr. l; and this morning, gr. lx. Let him take gr. xl. three times a day for three days; then gr. l three times a day.

Oct. 2.—No better. Let him take gr. lx three times a day for three days; then gr. lxx three times a day.

Oct. 9.—No better. Let him take gr. xc three times a day for three days; then gr. c.

Oct. 16.—No better. For the *first* time he complains of occasional nausea. Let him take gr. cx three times a day.

Oct. 23.—Let him take gr. cxx twice a day.

Oct. 30.—Much better. Let him take gr. cxv three times a day.

Nov. 6.—Worse again; sometimes feels a little nausea.

The man, I understand, was seen sometime afterwards not at all improved.

In extraordinary conditions of the system, it is well known that persons are little susceptible of ordinary impressions. Dr. Elliotson relates an example of insanity in which the patient took 80 or 90 grains of calomel night after night with no more effect than would have been produced on a person in health by a very few grains; and an instance of spasmodic asthma, in which a young lady not in the habit of taking opium, required above two table spoonfuls of laudanum to dissipate the paroxysm. The inertness of antimonial powder cannot, however be thus explained, because Dr. Elliotson observed that similar doses were just as well borne by persons little out of health; for instance, by those affected with cutaneous diseases. The ignorant are not contented with being cured by external applications, but are always urgent to take some internal medicine, and to several so circumstanced he administered the antimonial powder in doses of 90 grains, three times a day, and without any effect.

The magnitude of the doses precludes all probability of the power of the medicine being lost by habit, and in the very case I have transcribed, we see the dose was once increased in 24 hours from 30 to 60 grains; and on another occasion, at once, from 70 to 90 grains, without any sensible effect. Dr. Elliotson has furnished me with a case where the dose was at first so large, augmented so rapidly, and the patient's indisposition was so trifling, that nothing but the inertness of the preparation will account for the fact.

A footman in his family, aged 21 years, was seized Feb. 21, 1821, with the common symptoms of catarrh. He was ordered 10 grains of antimonial power at bed time.

Feb. 22.—No effect: ordered gr. xxx immediately. In the evening, there had been no effect: ordered gr. lx.

Feb. 23.—No effect; ordered gr. xc immediately. In the evening; an hour after taking the 90 grains, he vomited three times a large quantity of green bile.

He has not vomited nor felt sick since.

The bowels have been relieved once in the course of the day. As the stomach had been excited, Dr. Elliotson was desirous of learning whether a smaller dose would now produce nausea or vomiting, and he accordingly ordered the man gr. lx at bed time.

Feb. 24.—No effect whatever.

I must add that the medicine was procured for different patients from different shops, and that which was employed at St. Thomas's Hospital was supplied by Mr. Battley, of Fore-street; and some indeed was manufactured by him very carefully on purpose.

The facts which I have now mentioned are completely at variance with the opinions entertained by physicians of the highest character; I need only mention Dr. Duncan, who observes, "the oxide of antimony with phosphate of lime, howsoever prepared, is one of the best antimonials we possess. It is given as a diaphoretic in febrile diseases in doses of from three to eight grains repeated every third or fourth hour. In larger quantities, it operates as a purgative or emetic."

With this contradictory evidence in the subject, it appeared to me to be extremely desirable to examine more particularly into the nature of the oxide which enters into the composition of the antimonial powder. For after the well established fact that peroxide of antimony is nearly or totally inert, it appears to me that if proof could be obtained that the oxide of antimony is in this state, the deficiency of power in the pulvis antimonialis would be accounted for, at least in the cases mentioned by Dr. Elliotson, and although particular instances might occur of its proving extremely active, that circumstance would, I conceive, show that the preparation is worse than useless, because uncertain.

The Philosophical Transactions for 1801 contain a paper by Mr. Chenevix on this substance; and he has judiciously observed, that "every oxide of antimony with which we are acquainted is volatile at a high degree of heat: it would, therefore, be hazardous to assert, that it is possible to preserve always the same proportion of antimony, whatever care may be employed in directing the operation; and a dissimilarity in the chemical result must necessarily be attended with uncertainty in the medical application."

Dr. Pearson, who first analyzed James's Powder, of which the pulvis antimonialis is a professed imitation, appears to have considered these compounds as a triple salt, or a real ternary combination of phosphoric acid, lime, and oxide of antimony; whereas Mr. Chenevix considers the pulvis antimonialis as a mere mixture of the metallic oxide with the bone earth; for reasons which I shall presently mention, I confess I am entirely of the latter opinion.

In order to investigate the chemical nature of the pulvis antimonialis, I procured some at Apothecaries' Hall: into a retort containing 6 ounces of strong muriatic acid, I put 1000 grains of the powder and boiled the mixture for some hours, the muriatic acid which distilled, being returned into the retort. A large proportion of the powder remained undissolved by the acid, and when the solution had become clear, some of it was poured into water, but no precipitation whatever occurred.

As the quantity of muriatic acid employed was large, it may be supposed that the excess of it prevented the precipitation of any oxide of antimony that might have been dissolved; to obviate this objection, I decomposed the muriatic solution by carbonate of soda, and put the precipitate upon a filter; whilst moist, strong muriatic acid was poured upon it, and a solution with but little excess of acid was immediately obtained. I mixed 20 measures of water with one measure of this solution, but no precipitation took place, nor did the subsequent addition of a much larger quantity of water produce any effect; further to remove any objection as to the action of the muriatic acid in preventing that of the water, I made the following comparative experiment: to one measure of strong muriatic acid, I added 1-30th of its volume of a solution of muriate of antimony, and one measure of the above described solution; when 12 measures of water were put to this mixture, oxide of antimony was readily thrown down, notwithstanding the great excess of acid.

Although these experiments satisfied me that no oxide of antimony had been dissolved by the muriatic acid, and that it had taken up the phosphate of lime only, I submitted the muriatic solution to additional examination. It is well known that protoxide of antimony, when in a state of loose aggregation, is readily dissolved by potash, so that if the muriate of the metal be dropped into a solution of the alcali, the oxide at first precipitated from the acid is immediately redissolved by the potash: the muriatic solution obtained was therefore added to a solution of potash, precipitation immediately took place, but no excess of potash was capable of redissolving it, for when it was saturated with muriatic acid, no deposition took place: it is, therefore, evident that no oxide of antimony had been dissolved.

As then the muriatic solution contained merely phosphate of lime, it remained to examine the insoluble residuum; I had no doubt from its resisting the action of the muriatic acid, that it was entirely peroxide of antimony; it is, however, possible that it might be, as already alluded to, a triple compound of phosphoric acid, lime, and oxide of antimony, the latter being insoluble on account of its state of combination.

To determine this point, I mixed the insoluble residuum with carbonaceous matter, and subjected it to a red heat; when I found that it was readily dissolved by muriatic acid wi the assistance of heat, and that water threw down a cop white precipitate, which was evidently submuriate of prot of antimony. After filtration, I added ammonia to the sol but it occasioned the precipitation of a little peroxide of only. It appears then the residuum was merely oxide of mony in the highest state of oxidation; for if it had conta any phosphate of lime in combination, it would have been solved with the protoxide of antimony, and precipitate ammonia after the separation of the oxide by water.

For the purpose of determining the quantities of the pero of antimony and phosphate of lime contained in the pulvis monialis from Apothecaries' Hall, 200 grains were boiled long time in about three ounces of strong muriatic acid: 70 of peroxide of antimony were left undissolved, and conseque the powder consists of

Peroxide of antimony	35
Phosphate of lime	65
	100

I now procured some antimonial powder from another sou but of respectability equal to that above-mentioned; I could cover no difference in their appearance, but it was heavier that from the Hall in the proportion of about 100 to 85. this powder, I repeated experiments similar to those just deta and with similar results: it was a mere mixture of peroxid antimony and phosphate of lime, containing, however, ra more of the oxide. It consisted of

Peroxide of antimony	38
Phosphate of lime	62
	100

The experiments now detailed will, I think, sufficiently acc for the inertness of the pulvis antimonialis; it can only regarded as a mixture of phosphate of lime with the old dia retic antimony, a preparation of antimony now quite out of us account of its deficiency of power, and which is not likel be increased by admixture with phosphate of lime.

That the antimony should be thus converted into pero will be readily conceived, when it is remembered how sl metallic sulphurets part with the last portions of sulphur, animal matter with all the carbon it contains.

M. Chenevix has proposed to precipitate together proto of antimony and phosphate of lime; and provided a mixtur protoxide of antimony and phosphate of lime possessed any cacy which does not equally belong to oxide of antimony mixed with any other inert substance, his process is unquestionably a good one. It is, however, worthy of the consideration of medical men, whether tartarized antimony in small doses may not be advantageously substituted for every other antimonial preparation. It possesses the great advantages of being readily procured of certain and uniform power.

Article IX.

*An Account of the Principal Characters of the Earths and Metallic Oxides before the Blowpipe.**

[I am not aware that the characters of the earths and metallic oxides before the blowpipe have any where been so minutely and accurately given as by Mr. Children in his translation of Berzelius on the Use of the Blowpipe, &c. On this account, I have now copied them from that work, without any other alteration than that of divesting them of their synoptical form.—*Ed.*]

Abbreviations.—O. F. *Oxidating Flame.* R. F. *Reducing Flame.* = parts; *equal Parts of the Assay and Flux.* N. C. *Nitrate of Cobalt.* Fl. *Flaming.* C. *under the Column of either of the Fluxes means that the Support is Charcoal.* P. F. *Platina Foil.* P. W. *Platina Wire. A Brace* { *refers to the Substances in the first Column only, and includes all those which are contained in the Space it comprehends.*

Assay.	Heated alone on	
	Platina.	Charcoal.
lies		
ta	Infusible	Infusible
ydrate	Bubbles up and fuses	Is absorbed
rbonate	Fuses readily into a clear glass; enamel-white on cooling	Becomes caustic, and is absorbed
tita	Infusible	Infusible
ydrate	Like baryta	
rbonate	Fuses with moderate heat at the surface, great brilliancy; tinges strong R. F. red; becomes alkaline	
e	No change	
rbonate	Becomes caustic and alkaline; emits brilliant white light	
nesia	No change	No change
mina	No change	No change
ina	No change	No change
ia	No change	No change

From Mr. Children's Translation of "The Use of the Blowpipe in Chemical Analysis, and e Examination of Minerals; by J. J. Berzelius."

ASSAY.	HEATED ALONE ON	
	PLATINA.	CHARCOAL.
Zirconia	Infusible: emits intense light	Infusible; emits intense
Silica	No change	No change
Molybdic acid	F. fumes and fuses; brown-yellow on cooling; in R. F. blue; intense heat, brown	Fuses, and is absorbed, reduced
Tungstic acid	R. F. blackens, but not reduced	The same
Oxide of chrome	No change	The same
Antimony		Fuses readily; white fu condense into pearly cryst
Oxide of antimony	Fuses readily, and sublimes, in white fumes; *precipitated oxide*, burns like tinder into antimonious acid	Fuses readily, and re lours the flame greenish
Antimonious acid		Does not fuse, nor red a bright light
Antimonic acid		Whitens; is changed nious acid
Oxide of tellurium	F. fuses and fumes	Fuses, effervesces, and
Oxide of columbium	No change	The same
Oxide of titanium	No change	The same
Oxides of uranium		Peroxide becomes blackens, but does not fus
Oxides of cerium	Protoxide becomes peroxide	Peroxide does not alter
Oxide of manganese		Not fused; becomes b strong heat
Oxide of zinc	Yellow while hot; white when cold; does not fuse, but gives out great light when very hot, and white fumes, which condense like wool	
Oxide of cadmium	F. no change	Soon dissipates; leaves orange-yellow powder on coal
Oxide of iron	O. F. no change	R. F. blackens and bec netic
Oxide of cobalt	No change	The same
Oxide of nickel	No change	The same
Bismuth		Flies off in fumes, an mark with red, or ora which may be dissipated without giving colour to th
Oxide of bismuth	F. fuses readily, mass dark-brown, yellowish on cooling. In very intense heat reduces, and perforates the foil	Instantly reduced
Oxides of tin	Protoxide takes fire, and burns like tinder into peroxide	R. F. peroxide does not reduces in a strong prolon
Oxide of lead	Minium becomes black while hot; at incipient redness, changes to yellow oxide, fusible into orange-coloured glass.	Orange glass reduces i bule of lead
Oxide of copper		O. F. black globule; the charcoal; under surf R. F. reduces; with a gives a bead of metal
Mercury		
Oxide of silver	Instantly reduced	Instantly reduced
Gold		
Platina		
Iridium		
Rhodium		
Palladium		

SSAY.	HEATED WITH FLUXES.		
	SODA.	BORAX.	SALT OF PHOSPHORUS.
....			
....	Fuse, and are absorbed by the charcoal	Fuse readily with effervescence into a clear glass, which becomes opaque by Fl	As with borax, but foam and intumesce; end in a clear glass
e			
te			
....	No action on caustic strontita	Like baryta	tto
e			
te	= parts, fuses into a clear glass, becomes milky on cooling: in strong heat, bubbles, and absorbed by the charcoal		
....	No sensible quantity dissolved	Clear glass; opaque by Fl	Fuses in large quantity; clear glass
te		Fuses with effervescence; with more carbonate clear glass; crystallizes on cooling	Fuses with effervescence
....	No action	Like lime	Fuses readily; clear glass; saturated with magnesia, opaque on cooling
....	Swells up; forms an infusible compound	Fuses slowly; permanently clear glass	Permanently clear glass
....	No action	Clear glass, with a large proportion of the assay; opaque by Fl	As with borax
....	Like glucina	Like glucina	Like glucina
....	Similar to glucina	Like glucina	Like glucina, but dissolves more difficultly
....	Fuses with brisk effervescence; clear glass	Fuses very slowly; permanently clear glass	Very small portion dissolves; clear glass
acid	P. W. effervesces, clear glass; becomes milky on cooling. C. fuses, absorbed and reduced	P. W. clear glass in O. F. C. and in R. F. glass becomes dirty-brown, but not opaque	P. W. and in O. F. greenish glass while hot; colourless, cold In R. F. becomes opaque; dull blue while hot; clear and fine green on cooling C. same phenomena.
cid	P. W. dark-yellow glass, crystallizes on cooling; opaque white or yellowish C. and R. F. reduced	P. W. and O. F. clear glass; not opaque by Fl R. F. glass becomes yellow	O. F. yellowish glass R. F. fine blue glass
hrome.	P. W. and O. F. dark-orange glass; opaque and yellow on cooling. R. F. opaque; glass green on cooling C. absorbed, but not reduced	C. fuses difficultly, glass emerald-green; on P. W. and O. F. the colour flies, and glass becomes brown-yellow; on cooling, assumes a faint-green tinge	Green glass in both flames
....			
f antimony. ..	P. W. fuses; clear colourless glass becomes white on cooling C. is reduced	C. dissolves in large quantity; glass, yellowish, hot; almost colourless, cold. If saturated, part reduced and sublimed; strong R. F., the glass becomes opaque and greyish	P. W. and O. F. glass, yellowish, hot; colour flies on cooling

ASSAY.	HEATED WITH FLUXES.		
	SODA.	BORAX.	SALT OF PHOS
Antimonious acid....			
Antimonic acid......			
Oxide of tellurium.	P. W. colourless glass; white on cooling C. reduced	P. W. clear, colourless glass; white on cooling C. becomes grey and opaque	The same
Oxide of columbium....	Combines with effervescence, but not fused or reduced	Colourless, clear glass; becomes opaque by Fl	Fuses easil permanently cl
Oxide of titanium......	Fuses into a clear dark-yellow glass; white or grey-white on cooling, and crystallizes with evolution of great heat C. not reducible	P. W. fuses easily; glass, colourless; becomes milk-white by Fl R. F. glass assumes a dark amethyst colour, but transparent In large quantity on C. and R. F. glass, dull-yellow; when cold, deep-blue	O. F. clear, glass R. F. and o yellowish, hot ing, first red, fine bluish-viol
Oxides of uranium	C. brown yellow; not fused	P. W. dark-yellow glass; in R. F. becomes dirty-geeen	P. W. and yellow glass; c yellow, slightl C. and R. green glass
Oxides of cerium	C. not fused, soda absorbed; white or grey-white protoxide remains on the surface	O. F. fine red, or deep orange-yellow glass; colour flies on cooling; cold, yellowish tint. Enamel white by Fl. In R. F. loses its colour	O. F. fine colourless wh and quite limp
Oxide of manganese. ..	P. F. fuses, green glass, clear; cold, bluish-green C. not reduced	O. F. clear, amethyst colour glass; colour flies in R. F.	The same, - not so deep. in O. F. boils, off gas; in R quietly
Oxide of zinc	C. not fused, but reduced, with flame; white fumes, which cover the charcoal	O. F. fuses easily, clear glass becomes milky by Fl	Nearly the
Oxide of cadmium.	P. W. not fused C. reduced, sublimes, and leaves a circular yellowish mark	P. W. yellowish glass, colour flies on cooling; on C. glass bubbles, cadmium reduced, sublimes, and leaves yellow oxide	Dissolves quantity, cl on cooling, mi
Oxide of iron..........	C. absorbed and reduced; not fused	O. F. dull red glass becomes clear and yellowish, or colourless by cooling C. and R. F. bottle-green glass, or bluish-green	Similar to b
Oxide of cobalt........	P. W. pale-red by transmitted light; grey, cold	Fuses readily, deep-blue glass	The same, appears violet light
Oxide of nickel........	C. absorbed and reduced; not fused	O. F. orange-yellow, or reddish glass; becomes yellow, or nearly colourless, on cooling	As with bor colour flies al on cooling

.SSAY.	HEATED WITH FLUXES.		
	SODA.	BORAX.	SALT OF PHOSPHORUS
f *bismuth*		O. F. colourless glass R. F. partly reduced, muddy greyish glass	O. F. yellowish-brown glass, hot; colourless, but not quite clear, cold R. F. clear and colourless glass, hot; opaque and greyish-black, cold
tin	P. W. effervesces, tumified, infusible mass C. readily reduced	Fuses with great difficulty; permanently clear glass	As with borax
ead	P. W. clear glass becomes yellowish and opaque on cooling C. instantly reduced	P. W. clear glass, yellow, hot; on cooling, colourless C. flows over the surface and reduces	Clear colourless glass
opper	P.W. fine green glass, hot; on cooling, colourless and opaque C. absorbed and reduced	O. F. fine green glass, which in R. F. becomes colourless, hot; but cinnabar-red and opaque when solid	O. F. similar to borax; R. F. glass usually red, opaque, and like an enamel
ilver		O. F. glass becomes milky, or opaline, on cooling R. F. greyish	O. F. yellowish glass viewed by transmitted light by day, by candle-light reddish R. F. greyish

SSAY.	WITH OTHER REAGENTS.	REMARKS.
		The alkalies are not readily distinguishable by the blowpipe. *Lithia* leaves a dull yellow stain, when heated to redness on platina foil. *Ammonia* may be known by heating the assay with soda: it gives off a pungent vapour, which turns the yellow colour of moistened turmeric paper brown
	N. C.; a globule of different shades of red; colour flies on cooling	
e		
ite		
	N. C. exhibit a black, or greyish-black colour; do not fuse	
e		
ite		
	N. C. black or dark-grey mass, infusible	
ite		
	N. C.; flesh colour when quite cold	
	N. C.; fine blue glass, with strong heat when cold	The blue colour is only distinctly seen by day-light
	N. C.; black or dark grey mass	

ASSAY.	WITH OTHER REAGENTS.	REMARKS.
Silica	N. C.; blue glass when perfectly fused	The part not perfectly f nitrate of cobalt, has a re disagreeable colour
Molybdic acid		In the inclined glass tu gives off vapour, which partly on the tube as a whi partly on the assay in bril yellow crystals
Tungstic acid		If tungstic acid contain glass with salt of phosphor red in R. F. Tin makes blue.
Oxide of chrome		
Antimony		Antimony does not subl fusing point of glass. On when red, ignition continu neously. In a tube ope ends, it gives off white fum
Oxide of antimony		The oxide and acids of behave alike with the fl
Antimonious acid		
Antimonic acid		
Oxide of tellurium		Metallic tellurium heate matrass, first gives off va then a grey metallic sublim lurium. In a tube ope ends, emits abundant fum condense in a white fusible
Oxide of columbium		
Oxide of titanium	N. C. black, or greyish-black	For the rest of the phen the original work
Oxides of uranium		
Oxides of cerium		
Oxide of manganese		A very minute portion nese gives a green glass wit
Oxide of zinc		
Oxide of cadmium		
Oxide of iron		The reduction of iron peroxide to protoxide is faci tin
Oxide of cobalt	With subcarbonate of potassa, black glass when cold	
Oxide of nickel		
Bismuth		In a glass matrass does n at the fusing point of glas open tube scarcely gives fumes; the metal become with a dull-brown fused a slight yellowish tint, when
Oxide of bismuth		
Oxides of tin		
Oxide of lead		
Oxide of copper		
Mercury		All the compounds of m volatile; mixed with tin ings, and heated in a glass tallic mercury distils over.
Oxide of silver		
Gold		These metals have no the fluxes, which can onl detect the foreign metals be combined with. The examined by cupellation
Platina		
Iridium		
Rhodium		
Palladium		

ARTICLE X.

Astronomical Observations, 1822.
By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Aug. 28.	Immersion of Jupiter's third satellite	13h	29′	36·0″	Mean Time at Bushey.
		13	30	56·9	Mean Time at Greenwich.
Aug. 30.	Immersion of Jupiter's first satellite	11	58	18·6	Mean Time at Bushey.
		11	59	39·5	Mean Time at Greenwich.
Sept. 6.	Immersion of Jupiter's first satellite	13	51	56·5	Mean Time at Bushey.
		13	53	17·4	Mean Time at Greenwich.
Sept. 6.	Occultation of a small star by the moon. Immersion	14	03	27·5	Mean Time at Bushey.
Sept. 13.	Immersion of Jupiter's first or second satellite	15	45	22·0	Mean Time at Bushey.
		15	46	43·0	Mean Time at Greenwich.

N. B. The eclipses of the first and second satellites happened so near together, that while employed in writing down the first observation, the other took place.

ARTICLE XI.

Notice of Capt. Scoresby's Voyage to Greenland.
By T. S. Traill, MD.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Liverpool, Sept.* 20, 1823.

THE importance of the following communication will, I think, induce you, even thus late, to give it a place in the next number of the *Annals.*

The Baffin, the ship of our friend Capt. Scoresby, jun. arrived here on the 19th inst. from Greenland with 195 tons of blubber, the produce of nine whales. The Baffin obtained her cargo principally near the *east coast of Older West Greenland,* which has been also named *Lost Greenland,* from the long period in which it was invisible to Europeans. Within sight of this interesting country, Capt. Scoresby remained for three months, and in the intervals of the fishery employed himself in making observations on the geography and natural history of this hitherto almost unknown country. The result I understand is a real survey of the coast from lat. 75° N. down to 69°, comprising an extent (reckoning the various indentations and sinuosities observed) of about 800 miles! The coast visited by Capt. Scoresby is a continuation toward the north of that on which were planted the ancient colonies from Iceland, the fate of which is still veiled in such deep obscurity.

Capt. S. discovered several very extensive inlets; some of them indeed, it was ascertained, penetrate at least 60 miles within the general cut of the coast, and even then were without any visible termination. From the number and extent of these inlets; from the direction which some of them pursue; and from the many islands with which the coast is flanked, Capt. Scoresby believes the whole country to be a vast assemblage of islands; and he has grounds for concluding, that some of the inlets are passages communicating with Baffin's Bay!

But this is not all. The general form of the land was found to be so very unlike what is represented in our maritime charts, that only *three* places laid down could be recognised; and the error in the longitude of these, according to most of the charts, was no less than 15 degrees!

Capt. Scoresby landed on various parts of the coast, and in some of the bays; and on each visit to the shore discovered traces of inhabitants; some of them apparently recent. In one place he met with a considerable hamlet of deserted huts, among which were many graves. About this place he obtained many fragments of the domestic and fishing utensils of the inhabitants. Though the weather at sea was generally cold, the thermometer being about 38° or 40° Fahr. on the hills near this hamlet it was hot and sultry, and the air swarmed with musquitoes.

Capt. Scoresby has made a large collection of plants and of minerals, especially of rocks: he has also brought some zoological specimens. Animals of the higher orders were rare in that country; but he shot a white hare, and caught an animal of the genus *mus* with a short tail.

The high degree of interest which Capt. Scoresby's discoveries in this quarter must excite, will, I trust, induce him to publish his journal, which, according to his invariable laudable custom, is kept with great care.

To you who know the enterprising genius and philosophic spirit of Capt. Scoresby, his success will cause much more pleasure than surprise. When we see how much he has accomplished without any other means than that of a private individual engaged in an arduous and anxious occupation, we cannot help regretting that the government of this great commercial country has not seized the opportunity of employing the individual attention and talents of Capt. Scoresby in prosecuting his researches, no less conducive to the advancement of science, than to the glory of our country I am, dear Sir, yours, very faithfully,

THOMAS STEWART TRAILL.

ARTICLE XII.

*Curious Substance formed by some Chemical Changes from the Wine of the Sugar Cane.**

M. CLAMAN sent from Martinique to M. Vauquelin, for the purpose of analysis, a quantity of *vesou*, or the wine made from the sugar cane. It had been previously submitted to the means recommended by M. Apert for the preservation of vegetable substances; notwithstanding which, however, it had undergone some very remarkable alterations during the voyage.

In some of the bottles, it had fermented so as to produce alcohol, vinegar, and carbonic acid, but still containing a little sugar. In the greater number, it had entirely lost its saccharine taste, and a species of semitransparent gum had been formed in great quantity, and so thick as to quit the bottles with difficulty. Besides this portion separated from the liquor, much of the same substance remained in solution, from which it was precipitated by alcohol. The peculiar odour of vesou, however, was very perceptible. The contents of some bottles remained entirely fluid, acid, and saccharine, but still held much gummy matter in solution.

A portion of the vesou thus altered, which still retained a little sugar, was evaporated into a thick syrup, and the sugar crystallized. Another portion, which had been divested of its gum by alcohol, and of its acid by chalk, and which had likewise been reduced to a syrup, crystallized with more ease, and in greater quantity.

M. Vauquelin precipitated by alcohol two bottles of the thick vesou; washed and kneaded the gummy matter repeatedly with fresh portions of alcohol, pressed, and dried it. While moist, this substance is semitransparent, and of a greyish colour; it diminishes much in bulk by desiccation, and in that state is white, opaque, like the paste of starch, and has still a slight flavour of sugar.

It is very soluble in water, but the solution is always milky, even after filtration. If laid upon a burning coal, it becomes puffed up, is quickly carbonized, and emits a smell like that produced by sugar or gum; by distillation, it yields an acid, together with a little ammonia.

Four grammes of this substance were boiled for 10 or 12 hours, with 200 grammes of water, and 10 of sulphuric acid; the water lost by evaporation being from time to time replaced. The solution acquired a red colour, and on cooling deposited a substance of the same hue, which, after being washed and dried, gave out, when placed on a burning coal, an empyreumatic smell of animal matter. It is this animal substance, doubtless, which imparts to the aqueous solution its opaline appearance.

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* Ann. de Chim. xx. 93.

After the liquor had been filtered, in order to separate the red matter, it was saturated with carbonate of lime, the precipitated sulphate removed by a second filtration, and the fluid evaporated to the consistence of a thick syrup. This did not crystallize; it seemed to be more saccharine than the gum was prior to the operation, but was insoluble in alcohol. This gum, therefore, is not of the same kind as that obtained from starch, by the corresponding treatment with sulphuric acid.

Treated with nitric acid, it yielded much oxalic acid, and a small quantity of yellow bitter matter, but no mucic acid: this proves that it is not a true gum. A gramme of it burnt in a platinum crucible left a centigramme of ash, containing phosphate of lime, iron, and a particle of silica.

M. Vauquelin concludes, that this curious substance did not exist ready formed in the vesou, but that it was produced from the sugar contained in it.

Article XIII.

On a peculiar Sulphate of Alumina. By Richard Phillips, FRS. L. & E. &c.

Intending some time since to obtain a solution of sulphate of alumina in a state as nearly as possible approaching to saturation, I decomposed some alum by means of carbonate of soda, and after washing the precipitated alumina, I put it, while moist, into sulphuric acid, moderately diluted with water. Although the acid appeared to have taken up as nearly as much alumina as it was capable of dissolving, I nevertheless added alumina occasionally, until at last it remained floating in the solution.

I now separated the alumina undissolved, and filtered the solution, the specific gravity of which was very considerable; upon mixing a small quantity of it with water, I was surprised to find that it became turbid, and nearly as much so as when muriate of antimony is decomposed by it; indeed the alumina of a single drop of the solution was apparent in a pint of water. As far as I am acquainted with the properties of alumina, a sulphate decomposable by water has not been before observed; and it may be remarked that it is an additional point of resemblance between an earth and metallic oxides.

After the solution had been filtered, I observed that a deposit was almost immediately formed in the bottom of the bottle in which it was kept; this was separated, and a further quantity was obtained; indeed I found that during several months, the solution continued depositing, but the substance had not in any degree a crystalline form. Another property of this solution is worthy of notice: if some of it be put into a tube, and placed in water of the temperature of 160° to 170°, and probably even lower, it becomes opaque and thick in a few seconds; if, however, the tube and its contents be kept at the ordinary temperature of the air for several days, the precipitate is gradually redissolved, and the solution regains its transparency. It appears extremely singular that this solution should have required so long a time for its production, and perhaps still more so that the peculiar sulphate of alumina in question was not deposited as quickly as it was formed; yet I did not observe any disposition to deposit until after the removal of the excess of alumina floating in the solution. Not anticipating the spontaneous deposition which I have described, I did not take the specific gravity of the solution at its greatest density; but after it had continued depositing for several weeks, I found the specific gravity of the solution exceeded 1·120.

Although the solution of sulphate of alumina continued affording a deposit for many months, yet it did not appear to suffer any change of composition, for water added to it at this period continued to occasion precipitation, which it probably would not have done, if the deposit consisted of alumina combined with less acid than when in solution, for the excess of acid which must have remained in solution, would probably have prevented the precipitating action of the water.

As metallic oxides which are precipitated from acid solutions by water, usually contain a portion of the acid which held them in solution, there could be no doubt that the precipitate formed in this sulphate of alumina by water was a subsulphate, and I found it to be so, but I have not yet had leisure to determine its composition.

It is well known that it is extremely difficult to deprive alum of the whole of its sulphuric acid, and I found that alumina, even when precipitated from solution by excess of ammonia, and ignited, gave a precipitate with muriate of barytes when redissolved in an acid. It appeared to me, therefore, a question to be decided what quantity remains in combination with the alumina. I dissolved 1000 grains of alum in water, precipitated the alumina by carbonate of soda, and washed it with distilled water until it ceased to afford sulphuric acid, as determined by nitrate of barytes. I then dissolved the alumina in nitric acid, and added nitrate of barytes as long as precipitation occurred; the sulphate of barytes when dried weighed 24 grains, consequently the precipitated alumina contained 8·1 of sulphuric acid; and as 1000 of alum yield about 110 of alumina, I shall, in the experiments which I am going to state, deduct 7·36 per cent. from the precipitates of alumina, considering it as sulphuric acid.

To 736 grains of this solution, water was added as long as precipitation took place; the precipitate was dried by exposure to the air, and weighed 40 grains; 100 grains of the solution would, therefore, give 5·43 grains. I repeated this experiment with 1020 grains of the solution, which yielded 52·5 grains of precipitate dried as before; 100 would consequently have afforded 5·14 grains, giving a mean of 5·23 grains of subsulphate of alumina from 100 grains of the solution.

To determine the quantities of sulphuric acid and alumina which the precipitating sulphate of alumina contained, muriatic acid was added to 392 grains; this acid was of course employed to prevent the action of the water; nitrate of barytes was added, and 61 grains of ignited sulphate of barytes were obtained, equivalent to 15·56 per cent.; this experiment was repeated with 205 grains of the solution, and 31·9 of ignited sulphate of barytes were procured, giving also 15·56 per cent. As 118 of sulphate of barytes are equivalent to 40 of sulphuric acid, 15·56 will indicate 5·27, and consequently 100 grains of this solution contain 5·27 of sulphuric acid.

To 633 grains of the same solution, with which a little muriatic acid had been mixed, solution of carbonate of soda was added, until it was slightly in excess. The precipitated alumina, after being washed and ignited, weighed 36 grains; 100 grains of the solution would, therefore, have yielded 5·68 grains: this experiment was repeated with 625 grains of the solution, and 37 of ignited alumina were obtained; 100 of the solution would, therefore, have afforded 5·92, giving a mean of 5·8 of alumina for 100 of the solution. From this, however, for reasons already stated, we must deduct 7·36 per cent. which reduces it to 5·38.

It appears then that 100 grains of this solution contain

Sulphuric acid	5·27
Alumina	5·38

According to Dr. Thomson, hydrogen being 1, an atom of sulphuric acid is 40, and of alumina 18; and as 5·27 : 5·38 :: 40 : 40·83 the sulphate of alumina of this solution would not appear to be reducible to a probable definite compound; but I have already mentioned that a deposit was formed in it which appeared to be the same sulphate as that held in solution, for water continued to decompose the latter.

The deposited sulphate, when dried by exposure to air, is in some places opaque, and in others transparent; and when in the latter condition, it has the appearance of horn. To ascertain its composition, I dissolved 50 grains of it in dilute muriatic acid, and added a solution of muriate of barytes; 38·5 of ignited sulphate of barytes were obtained; therefore, 100 grains would have given 77 grains, equivalent to 26·10 of sulphuric acid. To ascertain the proportion of alumina, 100 grains dissolved in dilute muriatic acid were decomposed by carbonate of soda, and 28·8 of alumina remained after ignition. Deducting 7·36 per cent. from the alumina for sulphuric acid, this deposited sulphate of alumina very closely resembles that of the solution, and appears, therefore, to have been deposited without any decomposition. I have just shown that the solution consists of

Sulphuric acid	40·0
Alumina.	40·83

while the deposited sulphate is composed of

Sulphuric acid	40·0
Alumina.	40·92

From various considerations, more especially the constitution of alum, I am induced to differ from Dr. Thomson as to the weight of an atom of alumina. I shall take an early opportunity of returning to the subject; and, I think, I shall be able to show, contrary to his views, that alum contains a supersalt. Among other reasons for this opinion, I may state one experiment which I have very frequently repeated. If zinc filings be added to a solution of alum, they are gradually dissolved, but with sufficient rapidity to give out enough hydrogen gas to cause an explosion when a flame is presented.

According to my present opinion, an atom of alumina weighs 27, or one-half more than determined by Dr. Thomson. On this view the deposited sulphate of alumina which I have described will consist of

	By theory.	By exper.
2 atoms of sulphuric acid	40 × 2 = 80	80·00
3 atoms of alumina	27 × 3 ... = 81	81·75

I have already observed that when this solution is mixed with water, it is decomposed; and I have some reason for believing that the sulphate of alumina which remains in solution is that which with bisulphate of potash forms alum, the precipitate being, as I have ascertained, and indeed already mentioned, a subsulphate of alumina.

Article XIV.

Extract from a Memoir on the Composition of the Alkaline Sulphurets. By M. Berzelius.

M. Berzelius commences this paper with a history of the present state of our knowledge with respect to these compounds; and he then proceeds to detail the experiments which he has performed to elucidate the subject, beginning with

Experiments to determine whether the Sulphuret formed in the dry Way is a Sulphuret of the Oxide, or of the Metal.

If sulphuret of potassium can exist, it is evident it ought to be formed when sulphate of potash is decomposed; and after the solution of the compound in water, the nature of the result must depend upon the formation of a sulphuret of potash or potassium. To verify this, I made use of a small apparatus with an enameller's lamp, and so constructed that a current of hydrogen gas might be passed through it, while part of the apparatus was heated to redness by an argand spirit lamp. In this part one gramme (15·444 grains) of neutral sulphate of potash was introduced. This salt did not suffer any change for some time, but when the heat was raised, small red points were seen in parts which readily increased, and water was formed. The matter became black, and fused. The operation was continued as long as the gas introduced appeared to produce water, which was collected in muriate of lime. The salt, when cold, was of a fine cinnabar red colour; it had lost 0·315 gramme, and the water produced weighed 0·335 gramme. The red mass was easily dissolved by water, which became of a very light yellow colour. It deposited some silica yielded by the glass, muriatic acid evolved sulphuretted hydrogen with effervescence, and the solution was rendered slightly opaque by a little sulphur. Decomposed by muriatic acid, it gave with muriate of barytes 0·157 gramme of barytes, corresponding to 0·108 of sulphate of potash; the 0·335 gramme of water produced contain 0·298 of oxygen; but the sulphuric acid in one gramme of sulphate of potash contains only 0·275, and the potash 0·092 of oxygen. Then if it be remembered that there remained at the close of the experiment one-tenth of the salt which did not appear to have been decomposed, it will appear that about two-thirds of the potash were reduced to potassium, and that the remaining one-third combined with the glass when it lost its sulphur; one portion of which combined with the potassium, and the other was carried off by the hydrogen in the state of a white vapour; this caused the excess of loss of the salt, and which did not reappear in the oxygen of the water.

This experiment proves that the hepar contains sulphuret of potassium, seeing that if the combination of the sulphur with potash were possible, hydrogen gas certainly could not reduce this alkali in such a moderate degree of heat; but the loss suffered by the glass, throwing uncertainty upon the result of this experiment, I chose another method. In a similar apparatus, I reduced sulphate of potash by sulphuretted hydrogen, and I continued the operation as long as water escaped with the gas; it occupied three hours: some sulphur was deposited; but as soon as water ceased to be formed, the sulphur no longer separated from the gas. I suffered the operation to continue a quarter of an hour after this period.

One gramme of sulphate of potash was in this manner converted into 1·11 gramme of hepar. It was extremely fluid and black while hot; but on cooling, it became quite transparent, and of a deep red colour. It was readily dissolved by water; the solution was bright and yellow.

This solution was decomposed by muriatic acid, which precipitated a white powder without occasioning any evolution of gas. The fluid was heated to ebullition, and it then gave out a gas which was received in a solution of acetate of lead. After a moment's ebullition, a current of atmospheric air was passed over the liquid to expel the last portions of sulphuretted hydrogen. By these means a sulphuret of lead was obtained in the solution of the acetate, and which, after being washed and dried *in vacuo*, weighed 1·407 gramme, containing 0·189 of sulphur; but if all the alkali of one gramme of sulphate of potash were reduced to potassium, the sulphuretted hydrogen evolved ought to contain 0·184 of sulphur. This difference could only arise from an error of observation. The sulphur precipitated by the muriatic acid being washed and dried, weighed 0·488 gramme, and by fusing it lost no weight. After this precipitation, the liquid, when mixed with muriate of barytes, gave no sulphate.

One gramme of sulphate of potash contains 0·449 of potassium; supposing then that it is converted into sulphuret of potassium, the result of this is:

Potassium	44·9
Sulphur (precipitated)	48·8
Sulphur (in the sulphuretted hydrogen)	18·4
	112·1

That is to say, exceeding the hepar dissolved by 0·011 gramme, and undoubtedly derived from some error in the analysis. The hepar obtained was, therefore, sulphuret of potassium; but it is difficult to determine the degree of sulphuration. The sulphuretted hydrogen having lost sulphur during the formation of the hepar, this circumstance would seem to indicate a combination formed in determinate proportions which did not allow of its retaining the whole of the sulphur. In this case, it would be KS^7, and one gramme of sulphate of potash ought to weigh 1·093 after its decomposition by sulphuretted hydrogen. If the gas had deposited all its sulphur, the combination would have been KS^{10}. It appears then that in this operation three atoms of sulphur escape with the gaseous bodies; but I shall return hereafter to the different degrees of sulphuration of the potassium.

The same experiment was repeated, with this difference, that vapours of sulphuret of carbon were passed over the sulphate of potash. A gramme of this salt furnished 1·22 gramme of sulphuret of potassium, which, decomposed in the manner above stated, produced

Potassium	44·9
Sulphur (precipitated)	58·1
Sulphur (of the sulphuretted hydrogen)	18·4
	121·4

The liquor precipitated by muriatic acid did not contain any trace of sulphuric acid. The sulphuret of potassium approximated KS^8, whereas the combination resulting from the total decomposition of the sulphate of potash by the sulphuret of carbon ought to be, as in the preceding experiment, KS^{10} It should then weigh 119, instead of 122. Thus the actual result exceeds the eight atoms by the same quantity that the preceding result exceeded seven atoms. These experiments prove then most clearly, *that the hepar obtained was sulphuret of potassium of different degrees of sulphuration, and that by means of the presence of sulphur, a moderate degree of heat only is required to reduce potash to potassium by hydrogen or by carbon.* The glass was not acted upon in any of these experiments.

Five grammes of pure lime (deprived of water and carbonic acid) were introduced into a weighed porcelain tube, and exposed to a current of sulphuretted hydrogen gas. As soon as the atmospheric air had been driven out, the tube was heated to redness in that part which contained the lime. Aqueous vapours appeared, which were collected by muriate of lime. The operation was continued as long as the escape of water with the gas was perceived; the tube was suffered to cool, sulphuretted hydrogen gas being continually passed through it. I obtained 1·57 gramme of water, and there remained in the tube 6·41 grammes, which is very nearly the weight that ought to result from the conversion of lime into sulphuret of calcium, and the combination of the oxygen with the hydrogen of the sulphuretted hydrogen. The compound was dissolved in muriatic acid with the disengagement of sulphuretted hydrogen; muriate of barytes poured into the solution did not produce any precipitate. These experiments performed with an alkaline earth and an alkali prove then in a decided manner, *that the compounds hitherto regarded as alkaline or earthy sulphurets are compounds of sulphur with the metallic base of the alkali or earth.*

As hydrogen reduces sulphate of potash, and produces water, which evaporates, it is clear that at a high temperature, sulphur may also reduce the potash to sulphuret of potassium, and that sulphate of potash ought to be formed at the same time. This completely confirms the opinion of M. Vauquelin, with respect to what occurs when carbonate of potash is fused with sulphur.

This celebrated chemist states in his experiments upon the compounds of sulphur with the alkalies, that when potash unites with sulphur by fusion, a quantity of sulphuric acid is formed, the oxygen of which is equal to that of the potash, deducting, however, from the amount, the quantity of oxygen which exists in the potash combined with the sulphuric acid; this last portion forms one-fourth of the whole quantity of potash; so that the oxygen of the sulphuric acid can only constitute three-fourths of that which exists in the whole of the potash. In order to establish this fact, I prepared some hepar with one gramme of carbonate of potash, which was fused in a small retort with $1\frac{1}{2}$ its weight of sulphur.

The mixture was dissolved in boiling water, and precipitated by muriate of barytes, by which there were obtained in two experiments 0·421 gramme of sulphate of barytes. By calculation, 100 parts of subcarbonate of potash converted by this method into hepar, ought to give 42·15 parts of sulphate of barytes. These experiments prove then *that when subcarbonate of potash is fused with sulphur, one-fourth of the potash goes to form sulphate of potash, and the remaining three-fourths are converted into sulphuret of potassium;* this theorem may be employed in future in several calculations, and the accuracy of which it was proper to prove by experiment, although it was easy to discover it *à priori.*

II. *Experiments upon the different Proportions in which Potassium may be combined with Sulphur and with Sulphuretted Hydrogen.*

Before we proceed to examine the formation of hepar in the moist way, or by the intervention of water, we shall inquire what are the relations in which potassium may combine with sulphur and with sulphuretted hydrogen, which are necessary parts of the proposed examination.

When sulphate of potash is reduced by hydrogen or by carbon, sulphuret of potassium of the first degree of sulphuration is formed; that is to say, $K\,S^2$, which is proportional to the sulphate. It is difficult to obtain it pure. If the operation is performed in glass, it is acted upon; if in platina, a stronger sulphuret is formed, which is mixed with a compound of platina and potassium. When prepared in glass, the sulphuret has a pale cinnabar colour, and a crystalline fracture. It becomes dark when it is heated; it fuses at a heat below redness; and then it is black and opaque. Heated with excess of air, it does not inflame. All the properties of sulphuret of potassium sufficiently show that it is erroneous to attribute the ignition of pyrophorous to its presence; for it does not possess this power unless combined with a still more combustible body. It attracts moisture from the air, and dissolves into a yellowish fluid, which, when diluted with water, becomes colourless. It dissolves perfectly in alcohol. When moistened with water or alcohol, it does not become hot, which shows that the affinities acting in the solution are not very strong.

In order to discover the *maximum* of sulphur which combines with potassium, I fused 0·782 of a gramme of carbonate of potash with 1·5 gramme of sulphur in a small retort; the mixture was exposed to a moderate heat until the excess of sulphur was expelled. It then weighed 1·267 gramme. In the upper part of the retort, there was a small portion of hepar of a finer red colour, which, when dissolved in water, deposited sulphur. Its quantity was, however, so small, that its weight was not determined. The salt employed contained 0·5326 gramme of potash, of which one-fourth, equal to 0·13315 gramme, had formed sulphate of potash with 0·0458 gramme of sulphur, and with the oxygen of the remaining three-fourths. To determine the quantity of sulphur which was combined with the reduced potassium, the weight of the potash and that of the sulphur in the sulphuric acid, together 0·5784, must be deducted from 1·267. This quantity is 0·6886, which was combined with 0·3315 of potassium; that is to say, 100 parts of potassium had combined with 207·7 of sulphur; but this number is nearly equal to 10 atoms; for the weight of

$$K : 10\,S :: 100 : 205{\cdot}2$$

It appears then that 100 parts of subcarbonate of potash absorb as a *maximum* 93·9 parts of sulphur. The finer colour of the hepar, which was deposited upon the upper part of the retort, and which on solution deposited sulphur, induced me to think that a sulphuret in a still higher degree was formed, but which could not exist at a red heat, and which water decomposed, separating a portion of its sulphur.

(*To be continued.*)

ARTICLE XV.

ANALYSES OF BOOKS.

Transactions of the Cambridge Philosophical Society, Vol. I. Part II. 1822.

(*Concluded from* p. 63.)

V. *Notice of the Astronomical Tables of Mohammed Abibeker Al Farsi, two Copies of which are preserved in the Public Library of the University of Cambridge.* By Samuel Lee, MA. of Queen's College, Professor of Arabic in the University, and Secretary to the Cambridge Philosophical Society.

The author of this paper states, that as far as his researches have gone, the only notice of the work here alluded to is to be found in the Bibliotheque Orientale of d'Herbelot; and Mr. Lee concludes from the manner in which it is mentioned, that this author had never seen the work in question. "In presenting, therefore," says Mr. Lee, "to the Society a notice of a very scarce and valuable work on Arabian astronomy, I trust I shall do no more than what some of the most eminent writers in astronomy have often called for; and, in so doing, it is my intention to avoid prolixity, and to give such details from the preface of the work in question, and such extracts from the work itself, as may be interesting and useful." This paper, from its nature, scarcely admits of abridgment.

VI. *On Sounds excited in Hydrogen Gas.* By John Leslie, Esq. FRSE. &c. &c.

This paper is given in the last number of the *Annals*.

VII. *On the Connexion of Galvanism and Magnetism.* By the Rev. J. Cumming, MA. FRS. MGS. and Professor of Chemistry in the University of Cambridge.

Prof. Cumming commences this paper with observing, "that it has been remarked of the pile of Volta, that it stands unrivalled in the history of philosophy, as its discovery was not the result of accident, but the fruit of preconceived theory, without which it might have for ever remained unknown. But this, though the first, was not the only instance of the kind in the history of galvanism. The decomposition of the alkalies and the discovery of the close connexion, if not the identity of galvanism and electricity, were the results of experiments, which were not undertaken fortuitously, but successfully deduced from theoretical views. Another instance," says Mr. Cumming, "has been added of the verification of hypothesis by experiment, in Prof. Oersted's discovery of the action of the voltaic pile on the magnetic neeedle."

Prof. Cumming then proceeds to notice some facts which seemed to prove an internal connexion between magnetism and electricity, and he relates some fruitless experiments which had been instituted for the purpose of discovering whether magnetism, electricity, and galvanism, might not be identical. Prof. Cumming afterwards relates the various experiments which he had made on the subject of electro-magnetism, and describes the nature of the apparatus employed, and having determined the difference between galvanic magnetism and electricity, as to the power of being conducted, he was desirous of discovering whether there was any thing analogous in common magnetism. With this view he relates the following experiment: "I placed beneath the iron pendulum of a small clock a horse-shoe magnet, whose force coinciding with that of gravity, would accelerate the rate of the clock, by the going of which a measure would be afforded of the magnetic force exerted upon it. When the poles of the magnet were uncovered, the rate of the clock was accelerated from 10′ to 12′ in 24 hours; when they were connected by a piece of soft iron, the gain was not more than from 1′ to 2′; on filing away the middle of the iron, the rate was gradually accelerated, and when the central part was reduced to fine thread, the acceleration was nearly the same as when the poles were uncovered. When the poles were connected by a piece of iron bent down beneath the legs of the magnet, so that the length of the circuit between the poles was considerably increased, the rate of the clock was but little affected. It appears from this," says the Professor, "that the poles of the magnet were much more completely neutralized when the connexion between them was made through the longer but more capacious circuit, than when through the shorter and less capacious; and that in this respect common magnetism is analogous to that excited by the galvanic apparatus."

The author concludes this paper by observing, that there are, perhaps, few instances in the history of science, of nearer approximations to discovery than some of those connected with this subject. In the seventh volume of Nicholson's Journal, an account is given of an experiment for ascertaining the effects of galvanism upon a magnetic needle; which failed, as we now know, because the compass was placed *fortuitously* upon the pile, instead of being under or over the wires connecting its extremities.

When it was attempted to magnetize steel bars, by placing them in the circuit of the large electrical machine at Harlaem, it was observed that they became *most strongly* magnetic when the discharge was passed through them *transversely*.

VIII. *On the Application of Magnetism as a Measure of Electricity.* By the Rev. J. Cumming, MA. FRS. &c. &c.

Prof. Cumming observes, that the methods hitherto in use for ascertaining the quantity and intensity of the electricity produce either by friction or by galvanic action, are derived from its powe in decomposing water, or fusing metallic wire; both these methods he considers as presenting considerable practical difficulties, either when the quantity of electricity is small, or its intensity low. The discoveries of Oersted have, however, enabled Prof. Cumming to construct two instruments, one for discovering, and the other for measuring, galvanic electricity; and he is of opinion that their delicacy and precision scarcely admit of limitation. The construction of the first instrument is alluded to as having been already communicated to the Society; the following experiment is adduced in support of the opinion entertained of the delicacy of the instrument: A wire of zinc and another of platina, each 1-10th of an inch in diameter, were coated with sealing wax, so as to have merely their extremities exposed: on immersing them in a dilute acid, the circuit being at the same time completed through the galvanoscope, the needle deviated so decidedly, as to leave no doubt that a visible effect would have been produced by wires of less than half of the dimensions of those employed; as the compass used, though small, was not delicate. Prof. Cumming is of opinion, that the electricity developed by two metallic surfaces, each one 1-500th of a square inch, may be detected, and their relations to each other ascertained by this instrument.

Prof. Cumming states, that although he has not had sufficient leisure to form so complete a series of the electric relations of the metals towards each other, as this instrument is capable of doing, yet there are two instances which he mentions as being remarkable. On using two disks, one of iron, the other of steel, there was produced a decided deviation; since then the only difference in the metals arises from an alloy of from 1-60th to 1-100th part of the whole, it appears that this is sufficient to alter their electric relations. The powerful affinity of potassium for oxygen made it highly probable that in the galvanic circuit, it would become strongly negative with all the metals. On the first trial with disks of potassium and zinc, the potassium took fire before the effect could be observed; this difficulty was afterwards obviated by alloying it with mercury; on making the contact the needle deviated through nearly a right angle: the same effect was produced by copper.

With the assistance of the late Dr. Clarke and Mr. Lunn, the magnetic effects of atmospherical electricity were tried by a wire of about 100 yards in length, connected with a kite; a steel needle inclosed in a spiral wire was readily magnetized, but no deviation was caused in a compass placed beneath it.

Prof. Cumming concludes that galvanic magnetism is most readily made sensible by the deviation it causes in the compass needle; but the electrical by its power of communicating permanent magnetism.

For various other curious particulars, we must refer to the paper.

IX. *A Case of extensive Solution of the Stomach by t Gastric Fluid after Death.* By John Haviland, MD. Vice-P sident of the Cambridge Philosophical Society, and Regi Professor of Physic.

The subject of this case was a young man of about 20 yea of age, who died of fever, but had previously enjoyed goo health. The body was opened 12 hours after death, and th stomach on being examined after its removal from the bod afforded the following observations. The mucous membr appeared to be more red and vascular than usual throughout whole extent, and here and there were small spots of wh seemed to be extravasated blood, lying below the mucous co for these spots were not to be washed off, nor to be remov by the edge of the scalpel. There were two holes in the stomach, the larger very near to the cardiac end of the small curv ture, and on the posterior surface: this was more than an in in length, and about half that breadth. The other not far fro the former, also on the posterior surface, about the size of a si pence. The edges of these holes were smooth, well defined, an slightly elevated. The coats of the stomach were thin in ma other spots, and in one particular nothing was left but the pe toneum, the mucous and muscular coats being entirely destroye The hole in the diaphragm was through the muscular portio where it is of considerable thickness, and was large enough admit the end of the finger. There was no appearance of ulce ration or of pus adhering to the edges of this perforation of th diaphragm. Dr. Haviland concludes with stating the reason which induce him to believe that owing to the activity of th solvent power of the gastric juice, it sometimes not only corrode the parietes of this organ itself, but even the thick muscle of th diaphragm, and that within the space of 12 hours after death and he states the appearances presented by this case confi this opinion, which originated with Mr. J. Hunter.

X. *On the Physical Structure of the Lizard District in th County of Cornwall.* By the Rev. A. Sedgwick, MA. FRS. MGS. Woodwardian Professor, &c. &c.

In this paper, the author describes at considerable length a portion of the coast of Cornwall, which has always excited the attention of geologists. We regret that our limits will not allow us to do more than give a recapitulation of the statements which it contains, and this we shall do in the author's own words:

"From a general review of the facts already stated, it appears that a section made from the heights above Constantine to the mouth of the Helford river, and from thence to Old Lizard Head, in the general direction of the coast, would exhibit a series o formation nearly in the following order:

"1. Granite containing an excess of mica at its junction with the slate.

"2. Clay slate.

"3. Clay slate associated with greywacke slate, and containing subordinate parts, in which are conglomerate, common greywacke, and fine grained sandstone.

"4. Serpentine surmounted by granular diallage rocks, and amorphous greenstone passing into greenstone slate.

"5. An extensive porphyritic formation composed of felspar, diallage, and hornblende.

"6. Nearly compact masses formed of the same constituents, associated with a very large-grained diallage rock, and alternating with serpentine.

"7. Serpentine irregularly associated with saussurite, diallage rock, greenstone, greenstone porphyry, greenstone slate, and granular felspar.

"8. Greenstone slate.

"9. A formation apparently interlined both with the greenstone slate and the serpentine, and composed of chloritic slate (in one place associated with some thin beds of mica slate), talcose slate, and slaty felspar."

Prof. Sedgwick observes, that geologists have described two formations of serpentine, one belonging to primitive, the other to transition rocks; and he is of opinion that serpentine of the Lizard belongs to the latter class.

XI. *On Double Crystals of Fluor Spar.* By W. Whewell, MA. FRS. &c. &c.

This paper is accompanied by a plate, without which it would be unintelligible.

XII. *On an Improvement in the Apparatus for procuring Potassium.* By William Mundell, BD. Fellow of Queen's College.

This paper, which is short, we shall probably include among scientific notices on a future occasion.

XIII. *On a large Human Calculus in the Library of Trinity College.* By the Rev. J. Cumming, MA. FRS. MGS. &c. &c.

Of this calculus, which weighs 32 oz. 7 drs. we have already given some account in a former volume of the *Annals.*

XIV. *On a Dilatation of Ureters, supposed to have been caused by a Malformation of their Vesical Extremities.* By J. Okes, Esq.

This paper is purely surgical, and devoid of general interest.

XV. *Geological Description of Anglesea.* By J. S. Henslow, MA. FLS. MGS. Fellow of St. John's College, and Secretary to the Cambridge Philosophical Society.

This is a very elaborate communication, and is accompanied by a geological map, and illustrated by drawings of sections. Our limits will not allow us to give such an account of it as will do it justice.

XVI. *Some Observations on the Weather, accompanied by an extraordinary Depression of the Barometer during the Month of December,* 1821. By the Rev. John Hailstone, MA. FRS. &c.

It appears from Mr. Hailstone's statement, that on the 25th of

December, 1821, at three, a.m. the mercury stood at 28 inches, a degree of depression which he believes to be almost unprecedented in this climate.

XVII. *Notice of a remarkable Instance of Fossil Organic Remains found near Streatham, in the Isle of Ely.* By Dr. Frederick Thackeray.

This bone was picked up among the materials for forming the turnpike road in the neighbourhood of Ely. According to Dr. Thackeray, it "consists of limestone with a slight impurity of alumina and oxide of iron: the exterior of it retains some portion of phosphate of lime; and (what seems very singular) a minute quantity of animal matter, which was manifested by its peculiar fetid smell on being submitted to destructive distillation." It appears that a very considerable part of the skeleton of a mammoth was lately found in a gravel pit near Chatteris.

The History of British Birds; the Figures engraved on Wood, by T. Bewick; and a Supplement with additional Figures.

We ought perhaps to apologize for not sooner congratulating our readers on the appearance of this edition of the "History of British Birds." Indeed, our omitting to notice it arose at first from an impression, that in regard to a publication of such established repute, any thing beyond a bare announcement was superfluous. On reconsideration, however, it has occurred to us, that though this view may be correct as applied to the work itself, the public might not be disinclined to receive some account of the *Supplement,* now for the first time appended to it. Of the value and extent of that Supplement, we cannot give a better idea than by stating that it contains no less than *thirty-eight* cuts of birds not before figured by Mr. Bewick, delineated with all the spirit and accuracy for which he is so highly distinguished. By this large addition, he has advanced far towards completing the list not only of the native, but likewise of the accidental birds of Britain. There still remains a gap to be filled up; and although we certainly could have wished that he had left nothing for any other reaper in a field which he has so successfully cultivated, we are too well satisfied with what he has done to allow any feeling of disappointment to obtrude itself.

The term "British Birds" is in one respect very comprehensive, including three great divisions:—1st, Those birds which breed and remain with us all the year; 2dly, Those which only remain a certain time in obedience to the great and mysterious law of migration; 3dly, Those which alight or are driven on our shores by accident, or inclemency of weather. Of these, the first is obviously the one to which the distinction of *British* birds properly belongs. Naturalists, however, do not confine it to this class alone, and Mr. Bewick has, with similar views, given a place whenever he could procure a specimen, not only to the indigenous resident birds, but also to every casual visitor. A very considerable increase of this department of the British Fauna has been the consequence which has necessarily multiplied the labours of our ingenious artist, to an extent perhaps at first hardly anticipated.

It would be foreign to our present purpose to give any analysis of the principal work, public opinion having long since decided on its merits. But as an opportunity has never occurred of particularly adverting to it, we would here desire to express the gratification which repeated perusals of that instructive and entertaining production have afforded us. Its original pretensions, it is well known, were of the most unassuming kind. The "History of Quadrupeds," to which it was a sort of sequel, was intended as a succedaneum for the book of 300 animals. That paltry collection, however it might please the eye of childhood, was justly deemed an appendage fit for the nursery only. An elementary treatise which, while it inculcated a better taste and more correct knowledge, should by its amusing form imperceptibly win the minds of our youth to the study of natural history, was a desideratum. These objects, however, are so much better set forth by the author himself in his preface to the second volume of the present work, that, although it be stepping a little out of our way, we cannot help recurring to it. The passage to which we are about to quote contains a fair specimen of his general style, sentiments, and manner. "The great work," he says, "of forming the man cannot be begun too early. Among the many approved branches of instruction Natural History holds a distinguished rank. To enlarge on the advantages which are derivable from a knowledge of the creation, is surely not necessary; to become initiated into this knowledge, is to become enamoured of its charms; to attain the object in view requires but little previous study or labour; the road which leads to it soon becomes strewed with flowers, and ceases to fatigue: a flow is given to the imagination which banishes early prejudices and expands the ideas; and an endless fund of the most rational entertainment is spread out, which captivates the attention and exalts the mind. For the attainment of this science in any of its various departments, the foundation may be laid insensibly in youth, whereon a goodly superstructure of useful knowledge can easily be raised at a more advanced period. In whatever way indeed the varied objects of this beautiful world are viewed, they are readily understood by the contemplative mind; for they are found alike to be the visible words of God. Could mankind be prevailed upon to read a few lessons from the great book of nature, so amply spread out before them, they would clearly see the hand of Providence in every page."

"In ideas congenial with these, originated the first incitements which drew forth the histories of quadrupeds and British birds. From these humble attempts—for every attempt to depicture nature must fall short of the original—it is hoped that some useful instruction may be gathered, and at the same time a stimulus excited to further enquiry. To the rising generation these efforts to instruct and please are principally directed, and are set forth with an ardent wish that they may be found to deserve the notice of youth, and contribute to amuse and to inform them. May the reader, impressed with sentiments of humanity, on viewing the portraits, spare and protect the originals, and when these books shall become obsolete, or be lost in the revolution of time, may some other more able naturalist arise equally inclined to produce, better to supply their place."

Writing like this, it must be acknowledged, harmonizes admirably with the author's design, and is well calculated to promote the end at which he aims. His work, though in the first instance directed to the rising generation, has not been unproductive of advantage even to the initiated. In fact, it has been gradually becoming a book of authority and reference to the naturalists of every country. This character it owes scarcely less to the mass of select and valuable matter accumulated by its industrious author, than to his long established celebrity as a painter and engraver. It is true, there does not appear in it much display of that elaborate systematic research which some other works can boast, but the information contained in it is not on that account the less ample, precise, and authentic.

It is not our business to enter into the history of that beautiful and useful art which it was reserved for our able countryman to revive, and in reviving (dare we say?) to perfect. A few particulars, however, relating to it, we may notice, for the sake of correcting some misconceptions which strangely enough prevail even to the present time.

It has, for example, been repeatedly alleged, and is perhaps very generally credited, that wood is better adapted than copper to the pourtraying of animated nature. Whereas, the fact is quite the reverse. The superiority of copper is notorious, so far as regards softness of outline, delicacy and minuteness of execution, truth and fineness of ultimate effect. One striking advantage besides, which the artist on copper possesses over the wood engraver, is the knowledge of effect which he carries with him in his operations. While on wood effect can only be doubtfully appreciated, or rather guessed at, and to be fully ascertained must be proved at press; thus magnifying incalculably all the difficulties of the wood engraver, and leaving him almost wholly at the mercy of the printer. We have investigated this subject with some attention, and our conviction is that had Mr. B. practised copper engraving, he could with equal ease have excelled in it. Nay he might have surpassed even his present reputation on wood. This, we are aware, is saying much. Our opinion, however, is not lightly formed, for we have had an opportunity of examining some of his very few efforts on copper. Our opinion, is founded likewise on the general principle of the far higher capabilities inherent in the metallic surface, but more essentially still, on the entire mastery which he appears to possess over all the resources of his art in developing those capabilities.

Wood, however, has some compensating advantages of which it would seem he thought proper to avail himself. Possessing every suitable requisite for beauty, accuracy and effect, (at least in Mr. Bewick's hands,) it is also more *durable*, paradoxical though that may sound. This important quality, however, will at once be understood to be merely relative, and to depend chiefly on technical differences in the principles of the two arts, and particularly in the details of printing. At all events, it is sufficient to allow the multiplying of impressions, if not indefinitely, at least to an extent that ensures all the perpetuity that can reasonably be calculated on for any work. The degree in which this property exists in wood will appear when it is known that although several thousand copies of the engravings have now been thrown off, the cuts of the present edition are as perfect as those of the first. It will be made still farther evident from a circumstance which has lately come to our knowledge, that one wood cut has been known to stand upwards of nine hundred thousand impressions in the ordinary wear and tear of a newspaper press, without undergoing any material defacement.*

It results from these circumstances that the expense of publication and the cost to purchases are prevented from exceeding the bounds of moderation. Thus one of the primary objects of the work, that of combining pleasing and useful instruction with economy, is effectually accomplished, by bringing an interesting branch of study within the reach of many to whom the enormous price, according to any other mode of engraving, must have for ever rendered it inaccessible. Far then from quarrelling with Mr. Bewick for his preference of wood, we have cause to be pleased with his choice; for to that we owe the restoration of an important but neglected art, while an acquaintance with the particulars just enumerated tends greatly to enhance our estimate of those abilities which could, with materials intrinsically inferior, produce such results as he has done. These results, according to our judgment, fully warrant the opinion, that in truth and vigour of conception, boldness of outline, justness of proportion, fidelity and minuteness of delineation, adherence to physiognomy, attitude, character, manner, in short, in all that constitutes " nature's copy," Mr. Bewick has in his own particular province never been approached. It is both singular and satisfactory to observe that although the perceptions seldom become more acute with the lapse of years, he has in the supplement given unquestionable proofs that all his powers of design and execution continue unimpaired.

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* This curious fact may help to do away with some mistaken notions very prevalent with respect to the alleged superiority of the earlier editions, and the necessity of occasionally retouching the cuts.

The list in the supplement is, as we have already said, incomplete; not, we should hope, from any fault or neglect on the part of Mr. Bewick. We well know the difficulty in a provincial town of procuring good specimens, and we would heartily forgive him any reluctance he might feel to compromise his reputation by engraving from bad ones. The possessors of rare British birds do not seem to be yet thoroughly aware of the advantage they deny themselves of having their specimens imperishably preserved by *fac similes* from the graver of Mr. Bewick.

The birds are arranged somewhat promiscuously in the Supplement. We must content ourselves with presenting little more than a sort of *Catalogue Raisonnée,* interspered with such notices as do not appear in the text, but which may not be wholly uninteresting.

Land Birds.

Falcoæ Lagopus—Lin. (Gmel.) Rough Legged Falcon.—There is nothing very remarkable connected with the history of this bird. Montagu considers it the same as the *Falco Pinnatus.* In size, general aspect, and the fulness of the plumage, it resembles more the eagle than the falcon and hawk tribes. It is almost a pity that Linnæus had not originally separated by a genus *aquila,* the proper eagles from the smaller birds of prey. Cuvier has done something of this kind by a division into the *noble* and *ignoble,* beginning with the falcons, and placing the eagles *after* them as the *ignoble.* This arrangement is singularly fanciful, and proceeds on a fallacious assumption. A habit artificially imposed by man cannot constitute a ground of systematic distinction. Besides, it is the fierce untameable spirit of the eagle, which, sternly resisting all the attempts of man at subjugation, shows his true greatness of nature, and entitles him to the character of noble. We say nothing here of the taste displayed by the eminent naturalist in question, in making the hereditary and acknowledged monarch of the sky descend from that throne on which he has been seated by the common consent of mankind in all ages.

We are almost inclined to quarrel with Linnæus for not tampering a little with system, by placing him first in the list, and before the haggard, hungry, foul-feeding, fœtid race of vultures.

Strix Bubo—Lin. Le Duc ou Grand Duc—Buf. Eagle Owl, or great Eared Owl.—The most sage and dignified, if not the largest of the owl tribe, justifying by its sedate and thoughtful look the conceit of the ancients, which made it symbolical of wisdom. The cut affords a striking instance of the success of wood engraving in conveying all the difficulties of indistinctly variegated plumage. In the body of the work, we would particularly point out the white owl (*S. Flammea*), the long eared owl, bittern, night jar, tame duck, woodcock, and starling, as splendid examples of the same kind. We prefer the name eagle owl to any yet given, for reasons which will afterwards appear.

Strix Nyctea—Lin. Snowy Owl.—First made known to British Ornithology by Mr. Bullock, who in 1820, pursued one amongst some of the Orkney islands, but missed it. Thence passing over into the Zetland islands, he procured an adult male specimen, shot by Mr. L. Edmondston in the island of Unst, where it is understood to breed. According to the account given of it by Mr. Edmondston in the last volume of the Wernerian Transactions, its size must equal, if not exceed that of the former species, though Edwards and others are of a different opinion. Possibly the discrepance may have arisen from the individuals described having been of different sexes, the female being suspected by Mr. Edmondston to be the larger, as in the eagle and other rapacious birds. The Zetlandic name is *cat-yogle*, the appellation given to all owls in that part of the country. It is very rare, attaching itself to two or three districts only of the island, and affecting solitary, stony, and elevated places. Its aspect is comparatively lively, its form and manner rather elegant, and its flight less buoyant and more rapid than that of the other owls. It preys chiefly on sand-pipers, mice, and rabbits. The figures generally given of this bird denote difference of sex, immaturity of plumage, or the habit of changing with season or climate; for they appear more or less speckled with brown.

Strix Passerina—Lin. Petite Chouette—Buf. The Little Owl.—This was the *noctua minor* and *minima* of Gessner, Brisson, and the older naturalists. Linnæus characterizes it as *magnitudo passeris*, which must be a mistake, though it does vary much in size. It is the smallest of the *earless* branch, but certainly not of the *eared* branch of the owl family, for it is larger than the next in order, viz.

Strix Scops—Lin. Scops ou petit Duc—Buf. Little Horned Owl.—The engraving art scarcely furnishes any thing to surpass the softness and delicacy of touch displayed by Mr. Bewick in the plumage of this, the most diminutive of the owl kind. Indeed, we may remark that in the representation of plumage, he stands unrivalled. Artists in general content themselves with giving a rough resemblage of plumage. Mr. Bewick gives each particular feather, or at least all the important classes of feathers, as they are to be seen on the body of the bird. In the Gmelin edition of Linnæus' Systema Naturæ, this owl is said not to belong to Britain. It has only been lately arranged, and completes the series of British owls. This and the foregoing species exemplify in the most palpable manner, the impropriety of *making specific* character depend upon size. Any word, however unmeaning or cabalistical, would answer better for a trivial name than the degrees of comparison. It would be an act deserving the gratitude of the scientific to expunge such names from ornithological nomenclature, both systematic and vernacular.

Turdus Roseus—Lin. Merle couleur de Rose—Buf.—Rose-coloured Starling or Ouzel.—This is the male of a beautiful but very rare visitant. Mr. Bewick rather confounds names a little when he calls this a starling *or* ouzel, though it be in some measure intermediate. Linnæus ranks it as a *Turdus;* Pennant and Montagu call it an ouzel; Latham a thrush. With such authorities, the expedience of altering or multiplying names is not very apparent.

Turdus Solitarius — Lin. (Gmel) Merle Solitaire — Buf. Brown Starling or Solitary Thrush.—We have another misnomer here in starling. Although it must be allowed to approach near to the *stare* in some particulars, it is arranged in the system under the genus *turdus;* and Latham calls it a thrush. We would enter our protest against the use of equivocal or alternating English names. To indicate by a name that a bird is either a thrush *or* a starling, however closely those genera may be allied, is to confound identities.

Turdus Viscivorus — Lin. Draîne — Buf. Missel Thrush, Missel Bird or Shrite.—Our confidence in Mr. Bewick's general accuracy makes us suspect that the specimen from which this cut was taken, must have been in a state of imperfect plumage, for we have seen male birds on which the spots on the breast were much larger and much more beautifully clouded. The missel thrush is one of our earliest garden songsters, and we regret to learn that it begins to disappear from situations where it used to be well known.

Oriolus Galbula—Lin. Loriot — Buf. Golden Thrush, or Golden Oriole of Latham, and Golden Thrush of Edwards.—This is not a native, and probably never visits us but when forced by stress of weather. We object to the name of golden *thrush,* for the reasons already given, and prefer *oriole* of Linnæus, Pennant, and Latham. Buffon, it is true, places it as a connecting link between the two, and Edwards terms it a thrush; but their authority, in general respectable, is here equivocal in itself, and is, besides, borne down by superior weight.

Fringilla Cannabina — Lin. Grande Linotte des Vignes—Buf. Greater Red-pole, Greater Red-headed Linnet, or Brown Linnet.—Another instance of the impropriety alluded to of connecting specific distinction with relative bulk. Our own nomenclature, not that of Linnæus, is chargeable with it in the present case; but the objection applies to any language.

Linaria Montano. Linotte de Montagne—Buf. Mountain *Linnet or* Twite.—Is there any good reason for changing here *the generic term Fringilla* to Linaria? There is already in the

Syst. Nat. (Gmel) a *fringilla montium* which answers to this bird. We observe too in the body of the work, the same Latin name assigned to the common linnet, and to the lesser red-pole, as being on the authority of Linnæus.

Alauda Campestris—Lin. Spipolette—Buf. Field or Rock Lark.—We have some demur as to the precise place to be assigned to this bird. Established usage is obstinate; but, perhaps, a better arrangement,—of at least vernacular names, might be contrived for the individuals of this obscurely marked family. The distinction intended by Linnæus, and signified by Frisch, in the term *A. Novalium,* we believe, is, in the main, correct,—that it affects fallow or waste lands. This would lead to the expunging of *rock* lark, as the bird cannot be one and the other. Latham makes it *meadow* lark; perhaps, upon the whole, the best name of all. There is another lark that affects rocky situations; but whether these peculiarities may not sometimes arise from locality, or other circumstances, might admit of some doubt. In Egypt, it appears, the common sky-larks are called mountain birds; and very likely they may in that country affect high situations, which they certainly do not with us. Climate or season too often influences habit. Thus in winter we never see them in the air, and they utter only a faint note on the ground. In summer it is their lofty aerial position, and the long continued delightful warbling that point them out to observation.

Alauda Minor—Lin. Lesser Field Lark, or Tree Lark.—A similar incongruity of terms meets us here. Between *tree* lark, and *field* lark, there can be no affinity as regards habit. The authorities are in favour of its being called *field* lark, or *lesser* field lark,—that is, we suppose, reckoning the sky or common lark (A. Arvensis) to be the *greater* field lark. A difficulty might be got rid of by styling this bird the *A. Arvensis,* that is, field lark; and the sky lark might be called the *A. Vulgaris, canora,* or *ortherea.* There is a precedent for it in Gmelin's changing *linaria* to *linota* as the trivial name of the common linnet, though we suggest it with much hesitation. It would also free the subject from the embarrassment before urged, of making size a specific character. Such a practice is not admissible, even when the difference of size is striking, as in the bittern, and little bittern. But in a family like the present, the members of which vary in size so nicely as by a few eights of an inch, and are but faintly discriminated by plumage and other characters, the practice is calculated to destroy accuracy. Another proof, were any wanting, of the confusion thus created is to be found here. The present bird is styled the *A. Minor;* yet there is in the body of the work a description of the *A. Trivialis* or pipit, which is termed the *smallest* of the lark kind.

Musicapa Grisola—Lin. Gobe Mouche—Buf. Spotted Fly Catcher.

Sylvia Locustella—Lath. Fauvette Tacheteé—Buf.—This, we presume, is the bird of Latham's new genus, *sylvia,* which corresponds to the *motacilla nævia* of Gmelin's Linnæus,—that is, if the French synonyme be correctly understood. It is an elegant little bird, and most beautifully represented.

Motacilla Sylvia—Lin. Lesser White Throat.—Here must be an error. The M. Sylvia of Linnæus, is the *white throat,* figured and described at page 230 of the work. The *sylvia sylviella* of Latham answers to the white throat.

Parus Cristatus—Lin. Mesange Huppeé—Buf. Crested Titmouse.—This rare and handsome bird, Mr. B. informs us, was, with several others, lent to his work by the Honourable J. H. Liddle, of Ravensworth Castle, in the county of Durham, who possesses a valuable collection of our rarer birds. We are much gratified to see a taste for this fascinating study prevailing amongst others of our young nobility and gentry.

Tetrao Rufus—Lin. Perdrix Rouge—Buf. Guernsey Partridge or Red Legged Partridge.

Hirundo Pratincola—Lin. Perdrix de mer—Buf. Pratincole, Austrian Pratincole.—Linnæus places this bird among the Passeres, but describes it as intermediate between the grallæ and the swallows. Gmelin ranges it with the grallæ, under the new genus *glareola.*

Tringa Squatarola—Lin. Vanneau Gris—Buf. Grey Plover. —This, though a Tringa, (which, according to Mr. B.'s arrangement, forms one of the genera of water birds) is placed amongst the land birds, seemingly because the plover family had been by him included in that great division. It should have been a *charadrius,* if it be what Mr. B. considers it, really a plover, and not a sand-piper. Is there not some violence done here to the principles of classification, by separating the birds of the tringa genus from each other? Indeed the transformation into water birds of such birds as the oyster catcher, sanderling, curlew, whimbrel, woodcock, snipe, knot, heron, stork, crane, sand-pipers, &c. has long been a stumbling block in our way. They certainly do feed by the sea shore, by the margins of lakes and rivers, and in bogs, marshes, and fens; but be it always remembered that they do so with their feet on *terra firma.* None of their habitudes are aquatic in the strict, and we conceive the legitimate, sense of that term; they can swim with some facility, for a short time, if compelled, but it is upon the principle that a horse or an ox swims: it is never from choice. Place them on the water, suffer them not to leave it, or to touch the land, they could neither feed nor subsist. The *echassiers* (waders), as Cuvier properly enough terms them, may be deemed a sort of intermediate link; but if they belong to one element more than another, their structure, form, and habits, would seem to consign them to the land rather than to the water. The feet of the great plover, which bird is retained as a land bird, are much more webbed than those of many tringæ and scolopaces. The imperfect web may, while it serves to prevent them sinking deep in soft muddy places, be also a safeguard in the event of an accidental plunge into the water.

Water Birds.

Gallinula Foljambei—Montagu. Olivaceous Gallinale. *Gallinula Minuta.* Little Gallinule.—These two are not prominently discriminated. According to Montagu, the length of the former is 7¾ inches; that of the latter, 7½ inches; with the rest of the markings very much alike, or, at least, not more dissimilar than what might be supposed to arise from age, sex, or variety. The *rallus porzana—Lin.* the *R. Aquaticus minor*—Briss. seems, in some particulars, to come very near to the G. Foljambei, though we are far from proposing to strike Montagu's bird out of the list.

Ardea Minuta—Lin. Blougios de Suisse—Buf. Little Bittern.—It may be that we are accustomed to associate ideas of great size and height with all the members of this tall slender race; but the figure here conveys the notion of a larger bird than the description warrants, the body being the size of a thrush only. For this defect, however, there is no remedy but description. The bird here figured is a male; the one in the body of the work appears to have been a female.

Tringa Pygmæa—Mont. Ornith. Dict. Pigmy Sand-piper, or Pigmy Curlew.—This is the *Scolopax Pygmæa* of the Syst. Nat. (Gmel.) British Ornithology owes the present arrangement to Montagu. Whatever be the reasons for taking it from the genus scolopax, and ranking it as a tringa, its general aspect proclaims it more allied to the scolopax (or rather to the new genus *numenius*) or curlew.

Tringa Islandica—Lin. (Gm.) Red Sand-piper.

Tringa Hyperborea—Lin. Phalarope Cendré—Buf. Red-necked Phalarope.—Brisson, Buffon, Pennant, and Latham, call this simply the *red* phalarope. Such authorities should invariably give currency to a name, unless there be very urgent reasons indeed to the contrary.

Alca Pica—Lin. Petit Pingouin—Buf. Black Billed Awk.

Sterna Fissipes—Lin, Epouvantail—Buf. Black Tern.—Montagu identifies this with the sterna nigra.

Sterna Dugalli—Montagu. Roseate Tern.—Introduced by Montagu as an undescribed species: it was presented to him by Dr. Macdougall of Glasgow, by whom it was shot in the West Highlands of Scotland in 1812. It has been met with in the Fero Islands. We may observe, that the progressive stages, and the fluctuations (if any) of plumage of the tern family have not been so particularly noted as they might have been.

Larus Islandicas. Iceland Gull.—So called on the authority of Mr. L. Edmondston, by whom it was first recognised as a British bird in 1814. Mr. B.'s figure is evidently that of an immature bird. The trivial name of *zetlandicus* would, perhaps, have been as appropriate as the one given. It differs from all the larger species known in this country, in having the primary quills white to the very points. We should like to see a more detailed description of its specific character and habits.* A very ingenious and promising young naturalist informed us he had met with this bird lately, in Bantry Bay, Ireland. By the way, the more unfrequented parts of the Irish coast are likely to present a very fruitful and hitherto unexplained field for ornithological pursuits. And here we cannot avoid expressing our surprise and regret, that, considering the number of intelligent and well-educated men (especially in the medical profession) belonging to the sister island, so little should have been contributed by them to the advancement of this branch of natural history.

Larus Fuscus—Lin. Goéland a manteau gris brun—Buf. Herring Gull.—Though not the most numerous, yet the most generally disseminated, and familiarly known of the whole genus. This gull is described at length in the work; but we notice it here for the purpose of remarking that the portrait now given of it, will, we have little doubt, be allowed by every attentive observer to be admirable. The artist has, with his usual intuitive and felicitous tact, seized on the leading peculiarities in the character and manner of this bird,—particularly its eager watchfulness, readiness to take flight, and give warning to all in its neighbourhood of approaching danger. This singular fact, in the history of the herring gull, was first distinctly pointed out by Dr. Edmondston, in his View of the Zetland Islands. The instinct is quite unconnected with the protection of its nest or young ones. A similar instinct, but more limited in its object and operation, belongs to the L. Marinus, or black backed gull. It is a little curious that Sparrman takes notice of a species of *otis*, at the Cape of Good Hope, which he says, "conceals itself perfectly, with great art, till one comes pretty near to it, when, on a sudden, it soars aloft, and almost perpendicularly, into the air, with a sharp, hasty, and quivering scream, which is an alarm to the animals throughout the whole neighbourhood, discovering the approach of a sportsman or enemy of some kind or other." It seems uncertain whether the bird which now goes by the name of herring gull was the original *L. Fascus* of Linnæus, *brown*, being a colour nothing applicable to the back of this gull.

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* Mr. E. has lately sent to the Edinb. Museum an adult bird, with a particular *description.*

Lough Diver.—Much confusion prevails with regard to the distinction between this and the duck called castaneous; we do not pretend to unravel it. A description of the bird is given in the body of the work. The same may be said of the next:

Anas Canadensis—Lin. Oie à Cravate—Buf. Canada goose. An occasional visitor. It forms a suitable enough companion, though it greatly exceeds in size the one on the opposite page.

Anas Albifrons—Lin. Oie Rieuse—Buf. White fronted goose.

Anas Nyraca—Lin. (Gmel.) Castaneous duck. Mr. B. calls this the castaneous duck, we know not upon what authority. It seems to be the ferruginous duck of Pennant and Montagu, the tufted duck, var. O. of Latham, the olive tufted duck, Brit. Miscell. This it is to multiply names.

Anas Moschata—Lin. Canard musque—Buf. Musk duck. From the great size of this duck, the ease of rearing it, and its superiority to the common duck, it is rather surprising that it has not been more generally cultivated. It is pugnacious, especially the male, sparring and striking somewhat after the manner of our common poultry. Its gait is freer than the tame duck, the feet being placed nearer the centre of the body; its movements more alert, manner more restless, but more reserved. A useful hybrid is produced between it and the tame duck. It would form a valuable acquisition to rural and household economy; so probably might the eider be made were due pains taken.

The bird figured as the young of the *Larus Rissa—Lin.* or kittiwake,* will proclaim its lineage at once, without the aid of description. To those who with us have seen myriads of these beauteous inoffensive creatures peopling the stupendous cliffs that raise their front to heaven amid the north sea foam, this living breathing likeness will bring to mind many a pleasant scene "traversed so oft in life's morning march." The bird figured in Montagu's Appendix as the *L. Minutus* or little gull of Gmelin and Pallas, we agree with Mr. B. in considering an individual of this very species, in a different, perhaps more advanced stage of plumage. Indeed it is more than doubtful whether the *L. Minutus* has ever been authenticated as a British bird. We would here hazard a doubt as to the advantage resulting from that propensity which inclines some to seize upon and make a denizen, *nolens volens,* of every feathered straggler. We do not deem our *Fauna* so poor as to require this.

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* In the last volume of the Transactions of the Wernerian Natural History Society is an interesting account of *Foula*, one of the Zetland Islands, by Capt. Vetch, of the corps of Royal Engineers. In the ornithological part of it are some particulars respecting the kittiwake. He supposes it a "habit of this bird to prefer covered places of breeding," from their congregating in a natural arch. We are disposed to regard it as an instance of what we have already alluded to, namely locality modifying habit. The kittiwake in general affects mural, open, and exposed precipices for the purpose of nidification. Amongst others, we would instance *Noss Head* and *Barrafirth*, two of its most favourite resorts in the Zetland Islands.

We are disappointed to find the boldest and most elegant of the tribe, the arctic gull, *L. Parasiticus,* still wanting in Mr. B.'s work. It is neither so rare nor shy but a specimen might have been procured. Its more minute history is rather obscure. Some observations by Dr. Edmonston and Mr. L. Edmonston, in the later numbers of the Edin. Phil. Journal promise to throw light on it.* Enough, however, has appeared to prove Montagu's speculations to have been as well founded as they were acute, namely, that the *L. Crepidatus* and the young of the arctic gull are identical. Cuvier, in his *Regne Animal,* even so late as 1817, adopts from Brisson the genus *Stercoraires,* (the genus *Lestris* of Illiger,) in which he includes the L. Parasiticus and L. Crepidatus as distinct species. It is not a little singular to find so distinguished a writer not only retail the long received, but now universally exploded belief, in the unnatural instinct of these birds, but actually introduce a genus implying the existence of this habit.

From the size of the pair of black toed gulls in the Edin. Museum, we have sometimes fancied that they might be the young of the *Catarractes.* Has the young of the catarractes ever been precisely observed and discriminated?

The lesser black back, as it is called, *Larus Glaucus,* is also wanting to complete this numerous, changeful, and hitherto imperfectly discriminated tribe of birds.

Scolopax Canescens—Lin. (Gmel.) Cinereous godwit, This bird terminates the supplement. Mr. B. closes his description of it with some remarks on the confusion which prevails regarding the scolopax and tringa genera. The fluctuation of plumage, reserved habits, and near affinities of these numerous and ill-defined tribes will long oppose obstacles to the settlement of their respective claims. Nor will the absence of all precision in the language of colour, be one of the least obstacles to such settlement. Writers on natural history have, indeed, denied themselves a powerful auxiliary in so long hesitating to adopt Werner's Nomenclature of Colours, or some other constructed on a similar basis. We must give full credit to Mr. B. for the disinterestedness of his wishes on this point (excepting so far as he has to depend on the information of others), for no one stands less in need of colour to render his figures recognizable.

Mr. B. continues, we think judiciously, and at all events, consistently with the scheme of his work, to reject all synonymes but the most common and popular of the French from Buffon. Ornithology has, perhaps, from the very nature of the subject, suffered more than any other branch of zoology, from the laboured aggregation of synonymes, and the creation of new terms. To Linnæus this privilege might be allowed, were it only to show the chaos out of which he brought order; but where is the necessity of varying or repeating names for the fiftieth time? Not the least amusing part of the matter is that though authors predetermine to refuse to such synonyma all authority in their writings, a lurking *penchant* for display leads to their insertion, and to its unavoidable concomitants,—additional bulk and expense. When once the identity of a bird has been ascertained, and its place in the prevailing nomenclature universally acknowledged, and therefore fixed, all names but the Linnæan, or systematic, and the best established vernacular name, may be advantageously dispensed with; unless, perhaps, in the form of notes, to serve as the basis of future catalogues. We could say much on this subject, and on the principles and details of classification; but our limits do not admit of more than one or two remarks.

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* Capt. Vetch states several curious facts which manifest a nice and accurate observation of the habits and economy of the arctic gull.

To us it appears as though the aspect which natural history in general, and ornithology in particular, presents, however bright in some respects, were by no means encouraging, so far as classification is concerned. So many new arrangements and modifications (mere transpositions would have been immaterial) have been propounded and are daily pouring forth, that we look in vain for that universality of language which it was the primary object of Linnæus to establish, which was spoken by his illustrious pupils and their immediate successors, and enabled them to do the mighty things they have done towards the elucidation of nature. In Germany and the north of Europe, a close adherence to the Linnæan system;—amongst ourselves, the intermingling with that system alterations from every quarter;—in France the rejection of this and all other systems, and the creation of a new one, or rather a variety of systems different in their principles, and endlessly varied in their details,—promise such a store of glorious confusion for the naturalists of Europe, as will of itself long afford them matter of employment.

We may be wrong, but much of this "most admired disorder," we imagine, can be traced to a too early meddling with the Linnæan system, chiefly by two writers, whose popularity and influence were for a long time very considerable; but who, whatever their other merits might be, were not the best qualified either for rectifying old systems, or framing new. We mean Buffon and Pennant. The one affected to despise all system—the other entertained the utmost reverence for the Linnæn system. Nevertheless each must be indulged with a system of his own.

We would touch this subject with all the delicacy and deference which are due to the eminent authors who have so greatly illustrated it. We are not decrying innovation;—time and circumstances *suiting*, change is not only desirable, but indispensable. But we respectfully submit the following question to the consideration of the learned; and though at the risk of our notions being thought rather antiquated, we would desire to be understood as putting the case gravely and even strongly thus: "Would the branch of natural history of which we are now treating, have been injured or improved, supposing that naturalists had gone on conforming to the Linnæan arrangement, even with all its errors and imperfections (and they are doubtless manifold), until such time as more matured and definite information, a general wish felt, and inconvenience expressed by the scientific world, should, while it demanded, have sanctioned the introduction of a new or an improved system worthy of universal adoption?"

But we must desist, and bring to a conclusion an article which has expanded itself under our hands much beyond what was at first contemplated. The general tone of our remarks will have sufficiently evinced the high opinion we entertain of Mr. B.'s unpretending but meritorious labours. He has now all but perfected a work that must endure while a delightful branch of knowledge continues to be cultivated; nor do we despair of yet having to congratulate him on giving to it the finishing stroke of his graver.

Before taking leave, we would direct his attention to one work that would prove acceptable to every British naturalist, amateur, and sportsman. It is still a desideratum, viz: "A Manual of British Ornithology," with Mr. Bewick's cuts, and the leading generic and specific characters attached, in the manner of Smellie's Elements, or Turton's Manual. The materials for such or any similar work are in his hands, to be moulded into any form he may think fit.—[H.]

ARTICLE XVI.

Proceedings of Philosophical Societies.

GEOLOGICAL SOCIETY.

June 21.—A paper was read on the Rocks that occur in the Neighbourhood of Bovey Tracey, in Devonshire. By J. G. Croker, Esq.

The rocks which have been observed within this tract are granite, containing veins of tin and copper ores, a remarkable vein of micaceous iron ore, and its fissures tourmaline and apatite. Sienite containing ores of lead and copper, red sandstone, limestone of several varieties, and Bovey coal.

The author details the topographical situations and boundaries of these various rocks; but the assistance of the map by which the paper is accompanied, is necessary to render the descriptions fully intelligible.

"A letter from Sir Henry Bunbury, Bart. to Dr. Somerville, MGS. was read, giving an account of the strata pierced through in boring to the depth of 270 feet below the surface, at Mildenhall, in Suffolk. This boring was made in the hope of raising water to a higher level than that of the surface; but though the water filled the shaft, it did not rise above it.

"The substances passed through in this trial were the following:

	Feet.
1. Common white chalk, without flints.	35
2. Yellowish gritty chalk	5
3. Grey and hard chalk	136
4. Blue clay	54
5. Ditto darker and harder.	10
6. Ditto mixed with green sand.	10
7. Green sand with various fossils.	11
8. Blue clay with fossil shells.	9
	270

Among the fragments of fossils brought up by the boring machine were pieces of pentacrinite stalks, and fragments having the appearance of pyrites from the green sand.

A notice respecting the quartz rock of Bromsgrove Lickie, by Mr. James Yates, MGS. was read.

The quartz rock here referred to has been described by the Rev. Mr. Buckland, in the fifth volume of the Geological Transactions. The present notice details the characters and local positions of a series of specimens presented by the author to the Society.

The quartz rock passes on one hand into coarse friable limestone, in which the crystalline structure entirely disappears; and on the other, into a rock composed of minute quartz crystals.

The specimens illustrate this transition, and it is remarkable, that the crystalline varieties contain impressions of shells. In sinking a shaft to the depth of 40 or 50 yards on the eastern side of the Lower Lickie range, some of the usual beds of the coral formation were passed through; and at a considerable depth a limestone was found containing shells, which appear to belong to the genus anomia, beneath which was the quartz rock also containing impressions of shells of the same kind. These facts the author considers as sufficient to determine the class to which this rock belongs, and to place it decidedly among the transition series of formations.

The quartz rock of the Lickie is similar to that which occurs at the southern extremity of the Malvern Hills, and to the quartz grit of the Wrekin described by Mr. Aikin in the first volume of the Geological Transactions, and the author thinks it probable that it may rest on greenstone analogous to that of those two situations. The shell limestone also of Bromsgrove Lickie being identical with that which occupies a still higher portion in both those ridges.

Between Cradly and Stourbridge in the north-east angle of Worcestershire, fragments of the quartz rock rounded by attrition are found in a gravel pit imbedded in what seems to be a decomposing trap, probably identical with the slaty micaceous greenstone of the Wrekin. Portions of the trap fall off from the sides of the pit in flakes; but it is remarkable that the planes of separation pass without interruption through the pebbles of quartz rock and the trap; so that the sides consist of smooth vertical surfaces, like those which are said to occur in the cliffs of the pudding stone at Callender.

Article XVII.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Analysis of an Iron-ore from Brazil.*

Baron d'Eschwege, director-general of the mines of Brazil, has sent to M. Vauquelin for analysis a specimen of an iron-ore, which is found in detached pieces with micaceous iron-ore and topazes, in decomposed chlorite-islate, at Capao, near Villa Rica. The colour of this mineral is black; its fracture has a very strong lustre, resplendent as a mirror. When bruised, it is reduced into little micaceous laminæ; its powder is brown, and it is slightly attracted by the magnet. Sp. gr. 5·260. In muriatic acid it dissolves entirely, but is insoluble in nitric acid; exposed to a red heat, it augments a little in weight.

Dissolved in muriatic acid, it precipitates gold from its solution in the metallic state, proving that it contains protoxide of iron; but as a large quantity of the ore is requisite to precipitate a small quantity of gold, it cannot contain much of that compound. M. Vauquelin concludes, from an experiment in which 200 parts, calcined in a platinum crucible, augmented in weight 3 per cent., that this iron-ore consists of

Peroxide of iron....................	72
Protoxide of iron	28
	100

besides a small quantity of phosphoric acid and of manganese. It is probable, he observes, that all the iron-ores slightly attractable by the magnet, are similarly constituted.—(Ann. de Chim. &c. xx. p. 85.)

II. *Geology of the Sierra Nevada of Grenada.*

This chain of mountains, more elevated than the Pyrenees, and crowned, on some points, with eternal snows, has been geognostically examined by Don Josef Rodrigues, director of the Observatory of Madrid, and one of the most eminent disciples of the school of Freyberg. According to the geometric levelling of Don Clemente Rojas, the Picacho de Veleta is elevated 3,447 metres, or 11,309 feet above the sea: and the altitude of the Cerro de Mulhacen is 3,531 metres, or 11,585 feet.

The formations which constitute the entire mass of these mountains are primitive, and have great uniformity. They are mica-slates, which pass into gneiss and clay-state (phyllade, thonschiefer); and which contain subordinate beds of eupholite (serpentine, diallage rock), of quartz, and probably also of greenstone (diabase). There is neither granite nor true gneiss; nor are the fragments of those rocks found even in the neighbouring alluvial tracts. The existence of greenstone in subordinate beds is rendered extremely probable by the blocks of that substance which are dispersed around the principal chain. In this greenstone crystallized garnets are disseminated, as in that of the mica-slates collected by Humboldt in the chain of the littoral of Caracas. On the southern declivity of the Sierra Nevada, clay-slate reposes on the mica-state, and supports, in its turn, black transition limestones rich in sulphuret of lead.

It might appear from the abundance of the beds of greenstone, that the whole mass of these mountains belongs to the transition formation; but it must not be forgotten that the stanniferous granites of the Fichtelgebirge in Franconia also present beds and veins of greenstone, and that M. de Buch has discovered primitive eupholites in the North of Europe.* The strata of rocks which compose the Sierra Nevada are inclined in the form of tiles; that is to say, their direction is nearly parallel to that of the central chain, and they dip towards the north on the northern declivity, and towards the south on the southern. In the Alps, the strata are most frequently inclined towards the centre of the chain; on the coast of Italy they dip to the north. It will be interesting to the geognost to be well acquainted with the relation of the volcanic rocks of Cap de Gates to the intermediary and primitive formations of the Sierra Nevada. The tract which surrounds this chain is so elevated, that the upper platform of the tower of the Cathedral of Grenada is itself 784 metres, or 2,572 feet, above the marine level.—(Ann. de Chim. &c. xx. p. 99.)

III. *On the Preparation of Formic Acid from Tartaric Acid.*

Professor Dobereiner has found that when bitartrate of potash, or pure tartaric acid, is slightly heated with black oxide of manganese and water, a great quantity of carbonic acid escapes, and a sour colourless liquid distils, which is formic acid.

1. It is, even at the common temperature of the atmosphere, decomposed by concentrated sulphuric acid into oxide of carbon and water.

2. By nitrate of silver and pernitrate of mercury, when slightly heated, it is completely converted into carbonic acid, while the oxides are reduced to a metallic state.

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* It may be added, that diallage-rock and serpentine occur associated with gneiss, mica-slate, and other primitive rocks, in the Shetland Isles; and that the latter is also found in detached beds and masses in the granite of Aberdeenshire. There does not, indeed, appear to be any reason for supposing that the rocks of the Sierra above noticed belong to the transition class.—Ed.

3. It forms with barytes, oxide of lead, and oxide of copper, salts which have all the properties of formates.

What remains after the distillation in the retort, is, when tartaric acid has been used, tartrate and formate of manganese. If sulphuric acid be added, together with the black oxide of manganese, all the tartaric acid is decomposed into carbonic acid, water, and formic acid, and a greater quantity of the latter is obtained than in the former experiment. The best proportions for obtaining this acid are 78 parts of crystallized tartaric acid, 105 of black oxide of manganese, 115 of sulphuric acid, mixed with 2 or 3 parts of water.

Professor Dobereiner believes, that when nitric acid acts upon sugar, alcohol and formic acid are formed, and he finds that the easiest method of ascertaining its nature is to try the effect which sulphuric acid, and nitrate of silver, or pernitrate of mercury, have on the acid, either combined with water or with bases.

IV. *Bezoars voided by a Woman.*

The calculi called bezoars are found in the stomach and intestines of certain herbivorous animals, but had not been met with in those either of carnivorous animals or of man, until Dr. Champion, an eminent physician of Bar-le-duc, sent some for analysis to M. Henri Braconnot, which had frequently been voided, in a diurnal vomiting of blood, by an unmarried woman, whose menstruation was irregular, and whose urine had become much diminished in quantity before the evacuation of these concretions commenced.

These bezoars have the form of crisp almonds (*pralines*) and are as large as small hazel nuts; their surface is tubercular and coloured brownish red by the blood. Internally they are of a yellowish white, inclining to fallow, and appear to consist of brilliant crystalline grains; they do not present any concentric layers. They are usually of a close texture, but are sometimes cellular, like the marrow of bones; and may be cut with a knife like wood, of which they have also the aspect. At one of their extremities there is an infundibuliform depression, often filled with dried blood, which communicates with a tube extending throughout their length; this tube being sometimes partially or even wholly filled up. Two of them had cavities in the interior, like little geodes, but none offered a distinct nucleus; their specific gravity was above that of water.

These bezoars being boiled in water and the liquor evaporated, a slight residue was obtained, containing a free acid, the muriates of soda and potash, and a small quantity of animal matter. They were then treated with a solution of potash, which had little action upon them; a brown fluid resulted, however, in which muriatic acid occasioned a slight precipitate, that did not contain any uric acid. Every thing having thus been obtained from them that these solvents could extract, they were triturated with concentrated sulphuric acid, with which they produced a thick mucilage, that by solution in water and ebullition for some hours, was converted into sugar.

They were not acted upon by muriatic acid; by treatment with nitric acid 2 grammes of them yielded 0·4 grammes of oxalic acid, a small quantity of yellow bitter matter (*amer*), and an insoluble white substance resembling baked starch; this is readily soluble in ammonia, from which it is precipitated by acids in the form of a colourless jelly.

These bezoars are inflammable, but do not emit while burning the fetid smell which characterizes animal matter in combustion. Four grammes distilled in a glass retort, yielded 0·5 of a brown empyreumatic oil, and 1·7 of a yellowish fluid, which strongly reddened turnsol paper, and also contained ammonia. There remained in the retort 1·1 of charcoal, which left after combustion 0·14 of grey ash, affording to water the muriates of soda and potash, with traces of a sulphate and carbonate; and to muriatic acid some phosphate of lime; 0·02 grammes of silex remaining undissolved.

"It results from the preceding facts," says M. Braconnot, "that the bezoars vomited by the woman of Bar-le-duc have absolutely all the properties of wood; they bear a great resemblance to those which were found among the presents sent to France by the king of Persia, and which have been examined by M. Berthollet. But it is to be remarked that these oriental bezoars were easily soluble in potash, whilst ours are dissolved only in very small quantity by it. This would appear to render their resemblance to wood still more perfect." (Ann. de Chim. &c. xx. p. 194.)

V. *Fall of a Meteorite at Angers.*

The following particulars respecting this event are derived from a letter of M. Desvaux, keeper of the Museum of Natural History at Angers.

At a quarter after eight in the evening of the 3rd of June, in the present year, the sky being cloudless and the air calm, there was seen at many places, such as Loudun and Angers, towns sixteen leagues distant from each other, a vivid meteor to the south-east of the latter place, which remained visible for many seconds. To this succeeded a very loud detonation, followed by a rapid succession of reports of less intensity, resembling a running fire of musketry, and continuing for five or six seconds. This fire-ball, much nearer to Angers than to Saumur, appears to have had its centre of action over St. Jean-des-Mauvrets, a league and a half from Angers, on the left bank of the Loire. Luminous traces appeared in the atmosphere after the detonations, and a shower of stones descended, of which one, weighing 30 ounces, fell into a garden at Angers; the ground being hard, it made only a very slight hole, and being taken up at the moment of its fall had no particular heat; the temperature of the atmosphere was betweeen 81° and 82° Fahr.

This meteorite, which appears to have been the only one of the shower that had been taken up, is described as being an irregular angular fragment, evidently a portion of a larger mass, invested with a brown black crust, on part of which is a bubble; and presenting, interiorly, the same aspect and structure as the stones which fell at l'Aigle in 1802.

An interesting account of the meteor itself is given in a letter addressed to M. Arago by M. Boisgiraud, sen. Professor of the Physical Sciences at the Royal College of Poitiers; of this the following is an abstract:

There was seen at Poitiers, at eight o'clock, in the evening of the 3rd of June, a beautiful *falling star* in the NNE, consequently near the magnetic meridian; it resembled in its brilliancy and in the nature of its light, the fire-work called a Roman candle. It left after it a luminous rectilinear train, which, attenuating towards the top, increased in diameter as far as a point a little above its lower extremity. This point, more luminous and of greater diameter than the rest, also remained for a much longer time; it subtended an angle sufficiently sensible. The inferior extremity of this train was in the constellation Auriga, passing between the stars Capella and β; by degrees it altered in form, and presented, nearly, the aspect of the projection of a helix traced upon a cylinder. The extent of this helix diminished in proportion as its diameter augmented, and its brilliancy sensibly decreased at the same time. After some minutes its continuity ceased, and it became divided into two branches, the superior of which contained the greater portion of the curve, and both extremities of each branch were directed towards the west. The upper branch continued slowly to diminish in brilliancy, and without change of place or further alteration of form it ceased to be visible in ten or twelve minutes after its first appearance. The inferior branch still presented an irregular curve, and after the lapse of some minutes, nothing of it remained except the brightest point, or nucleus, the lustre of which became slowly extinguished. The position of this nucleus with respect to the two stars above mentioned, as far as could be judged without an instrument, appeared to be invariable; notwithstanding that the phenomenon continued for a quarter of an hour, and that the diurnal motion of the stars had been sufficiently sensible. (Ann. de Chim. &c. xx. p. 89.)

VI. *Case of a Man swallowing Clasp Knives.*

Dr. Marcet has given a curious and detailed account of this case in the 12th vol. of the Medico-Chirurgical Transactions, from which we extract the following particulars:—

In the month of June, 1799, John Cummings, an American sailor, about twenty-three years of age, being with his ship on the coast of France, and having gone on shore with some of his ship-mates, about two miles from the town of Havre de Grace, he and his party directed their course towards a tent, which they saw in a field, with a crowd of people round it. Being told that a play was acting there, they entered, and found in the tent a mountebank, who was entertaining the audience by pretending to swallow clasp-knives. Having returned on board, and one of the party having related to the ship's company the story of the knives, Cummings, after drinking freely, boasted that he could swallow knives as well as the Frenchman. He was taken on his word and challenged to do it. Thus pressed, and though (as he candidly acknowledged in his narrative) "not particularly anxious to take the job in hand, he did not like to go against his word, and having a good supply of grog inwardly," he took his own pocket-knife, and on trying to swallow it "it slipped down his throat with great ease, and by the assistance of some drink and the weight of the knife," it was conveyed into his stomach. The spectators, however, were not satisfied with one experiment, and asked the operator "whether he could swallow more?" his answer was, "all the knives on board the ship;" upon which, three knives were immediately produced, which were swallowed in the same way as the former; and, "by this bold attempt of a drunken man," (to use his own expressions) "the company was well entertained for that night." The next morning he had a motion, which presented nothing extraordinary; and in the afternoon he had another, with which he passed one knife, which however was not the one that he had swallowed the first. The next day he passed two knives at once, one of which was the first, which he had missed the day before. The fourth never came away, to his knowledge, and he never felt any inconvenience from it. After this great performance, he thought no more of swallowing knives for the space of six years.

In the month of March 1805, being then at Boston, in America, he was one day tempted, while drinking with a party of sailors, to boast of his former exploits, adding that he was the same man still, and ready to repeat his performance; upon which a small knife was produced, which he instantly swallowed. In the course of that evening he swallowed five more. The next morning crowds of visitors came to see him; and in the course of that day he was induced to swallow eight knives more, making in all fourteen.

This time, however, he paid dearly for his frolic; for he was seized the next morning with constant vomiting, and pain in his stomach, which made it necessary to carry him to Charleston hospital, whereat, as he expresses it, "betwixt that period and the 28th of the following month, he was safely delivered of his cargo."

The next day he sailed for France, on board a brig, with which he parted there, and embarked on board the Betty of Philadelphia, to return to America. But on his passage, the vessel, which was probably carrying on some illicit traffic, was taken by his Majesty's ship the Isis, of fifty guns, and sent to St. John's, Newfoundland, where she was condemned, while he himself was pressed and sent to England on board the Isis. One day, while at Spithead, where the ship lay some time, having got drunk, and, as usual, renewed the topic of his former follies, he was once more challenged to repeat the experiment, and again complied, "disdaining," as he says, "to be worse than his word." This took place on the 4th of December 1805, and in the course of that night he swallowed five knives. On the next morning the ship's company having expressed a great desire to see him repeat the performance, he complied with his usual readiness, and "by the encouragement of the people, and the assistance of good grog," he swallowed that day, as he distinctly recollects, nine clasp-knives, some of which were very large; and he was afterwards assured, by the spectators, that he had swallowed four more, which, however, he declares he knew nothing about, being, no doubt, at this period of the business, too much intoxicated to have any recollection of what was passing. This, however, is the last performance we have to record; it made a total of at least thirty-five knives, swallowed at different times, and we shall see that it was this last attempt which ultimately put an end to his existence.

On the following day, 6th of December, feeling much indisposed, he applied to the surgeon of the ship, Dr. Lara, who by a strict inquiry, satisfied himself of the truth of the above statement; and, as the patient himself thankfully observes, administered some medicines, and paid great attention to his case, but no relief was obtained.* At last, about three months afterwards, having taken a quantity of oil, he felt the knives (as he expressed) it "dropping down his bowels;" after which, though he does not mention their being actually discharged, he became easier, and continued so till the 4th of June following (1806), when he vomited one side of the handle of a knife, which was recognized by one of the crew to whom it had belonged. In the month of November of the same year, he passed several fragments of knives, and some more in February 1807. In June of the same year, he was discharged from his ship as incurable; immediately after which, he came to London, where he became a patient of Dr. Babington, in Guy's hospital. He was discharged after a few days, his story appearing altogether incredible, but was re-admitted by the same physician, in the month of August, his health during this period having evidently become much worse. It was probably at this time that the unfortunate sufferer wrote his narrative, which terminates at his second admission into the hospital. I find, however, by the hospital records, that, on the 28th of October he was discharged in an improved state; and he did not appear again at the hospital till September 1808, that is, after an interval of nearly a year since his former application. He now became a patient of Dr. Curry, under whose care he remained, gradually and miserably sinking under his sufferings, till March 1809, when he died, in a state of extreme emaciation.

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* An interesting letter from Dr. Lara, was found among Dr. Currys's papers, which supplies some of the particulars respecting the patient's illness, while on board the Isis;

VII. *New Analyses of the Amphibolic Minerals; by P. A. de Bonsdorff.*

1. Grammatite, from a quarry of primitive limestone at Gullsjö in Wermeland. Crystallized without secondary facets, the obtuse angle measuring 124° 33′½; colourless. Fuses readily before the blowpipe with a strong ebullition.

2. Grammatite from Fahlun. Forming tetragonal prisms imbedded in talc; colour honey-yellow, harder than the amphiboles, and, in general, giving sparks with steel. More difficultly fusible before the blowpipe than the other minerals here described, but with a considerable ebullition.

3. Vitreous Actinote from the iron mines of Taberg, in Wermeland; accompanied with oxidulous iron ore, green foliated talc, and a little calcareous spar. Scopiform, straight or curved; and passes by an insensible transition from green rays of a considerable size to very fine white fibres, having perfectly the aspect of asbest; it is very brittle, and has a very strong vitreous lustre. The deeply-striated surfaces of the crystals do not permit an exact determination of the angles. Before the blowpipe, it presents, in the exterior flame, little shining bubbles, accompanied with a kind of phosphorescence: in the interior flame it melts with difficulty into an opaque glass.

4. Asbest of Tarentaise in Savoy. White, flexible, and elastic. In the exterior flame of the blowpipe, it presents a great quantity of incandescent bubbles; but in the interior it melts tranquilly.

5. Bright grey grammatite, in tetragonal prisms, imbedded in carbonate of lime, from Aker in Sudermanland; accompanied by spinel, mica, and compact paranthine; colour bright grey, tinged with red; translucid. Obtuse angle 124° 34′. Before the blowpipe, in the ex-

and the close coincidence between Dr. Lara's statement and the account of the patient *himself*, forms a chain of evidence of the most perfect and conclusive kind.

terior flame, it becomes pale, and presents bubbles from time to time; in the interior flame, before a strong blast, it melts with considerable ebullition.

6. Dark brownish grey grammatite found at Aker in the same limestone, and under the same circumstances as the preceding, with which also it agrees in characters, except as to colour. It is sometimes found crystallized with secondary facets. The oblique angle measures 124° 31′.

7. Amphibole from the iron-mines of Nordmark, in Wermeland, where it is accompanied by magnetic iron ore, and dark green chlorite, and sometimes by colourless apatite. Its colour is black or greenish black, it is opaque, and its powder is green. Reduced to a coarse powder it is attracted by the magnet, and, after calcination, in fragments of considerable size. Angle 124° 28′½. Characters before the blowpipe the same as those of the actinote of Taberg.

8. Amphibole from Yogelsburg in Wetterau; matrix probably a basalt. Colour black or brownish black, by reflected light; but reddish brown by transmitted light; translucid, powder rust coloured. Crystallized in hexaedral prisms, with facets on the summits; the oblique angle of the primitive prism 124° 32′½. Character before the blowpipe as in the preceding, but it is more fusible than any of the varieties above described.

9. and 10. Pargasite and the Amphibole of Pargas. These minerals are found in the quarries of carbonate of lime at Pargas in Finland, and it is remarkable that notwithstanding their analogy in composition, they never accompany each other, and are never observed to pass into each other. The colour of pargasite is green; that of the amphibole is perfectly black. They are found in grains; and in hexaedral prisms, having all the facets of amphibole, as well primitive as secondary; but the crystallization of the black variety is always the most complete. The green variety is much more translucid than the other. They both fuse before the blowpipe with a violent ebullition.

	1	2	3	4	5	6	7	8	9*	10
Silica	60·31	60·10	59·75	58·20	56·24	47·21	48·83	42·24	46·26	45·69
Magnesia	24·23	24·31	21·10	22·10	24·13	21·86	13·61	13·74	19·03	18·79
Lime	13.66	12·73	14·25	15·55	12·95	12·73	10·16	12·24	13·96	13·83
Alumina	0.26	0·42	—	0·14	4·32	13·94	7·48	13·92	11·48	12·18
Protoxide of iron ..	0·15	1·00	3·95	3·08	1·00	2·28	18·75	14·59	3·48	7·32
Protox. of mangan.	—	0.47	0·31	0·21	0·26	0·57	1·15	0·37	0·36	0·22
Fluoric acid	0·94	0·83	0·76	0·66	0·78	0·90	0·41	traces	1·60	1·50
Water	0·10	0·15	—	0·14	0·50	0·44	0·50	—	0·61	—
	99·95	100·01	100·12	100·08	100.18	99·93	100·89	97·10	96·78	99.53

The angles of the crystals were measured by M. Mitscherlich. M. de Bonsdorff observes, that it is a fact worthy of attention, that the amphiboles which contain alumina, or those of which the composition is most complicated, are almost always found crystallized with secondary facets; while the grammatites, of more simple constitution, present only primitive facets.—(Ann. de Chim. &c. xx. p. 5. From the Memoirs of the Academy of Sciences at Stockholm.)

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* *This mineral also yielded* 0·42 *of* "*substance mélangée.*"

VIII. *Electro-magnetic Experiment.*

M. Nordenskiold of Abo, now in this country, has communicated to me the following curious and simple experiment of Dr. Seebeck of Berlin. Take a bar of antimony about eight inches long, and half an inch square, connect its extremities by twisting a piece of brass wire round them so as to form a loop, each end of the bar having several coils of the wire. If one of the extremities be heated for a short time, with a spirit lamp, electro-magnetic phenomena may be exhibited in every part of it.

Article XVIII.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

Mr. Physick, Sculptor, will publish twelve Subjects on Utero-Gestation, which he has modelled from the Originals of the late Dr. Smellie, and which are now in the possession of H. G. Clough, Esq. These Models are coloured from Nature, and will be open for inspection every Monday and Thursday (commencing the 3d October) between the hours of one and four, at 23, Spring-street, Portman-square.

A Treatise on Dislocations and Fractures of Joints. By Sir Astley Cooper, Bart. F.R.S. 4to. with plates.

Dr. John Boron is about to publish, Illustrations of the Inquiry respecting Tuberculous Diseases, with coloured Engravings.

Joseph Swan, Esq. has in the Press, An Inquiry into the Action of Mercury on the Living Body.

Mr. Henry Mayo, Surgeon and Lecturer on Anatomy is preparing for publication, Anatomical and Physiological Commentaries.

Mr. W. Wallace, Surgeon and Lecturer on Anatomy is printing a System of General Anatomy, in an 8vo. volume.

Article XIX.

NEW PATENTS.

J. Bold, West-street, Long-lane, Bermondsey, printer, for improvements in printing.—July 4.

Jonas and John Hobson, Mythom Bridge, Yorkshire, woollen-manufacturers, for new machinery for a more effectual and expeditious mode of shearing, cutting, and finishing woollen cloth, &c. which require the use of shears.—July 27.

J. Stanley, Manchester, smith, for machinery calculated for a more efficacious mode of supplying furnaces with fuel, whereby a considerable reduction in coals and labour is effected, as also in the appearance of smoke.—July 27.

J. Pearse, Tavistock, iromonger, for improvements in the construction and manufacture of spring-jacks, &c.—July 27.

ARTICLE XX.

METEOROLOGICAL TABLE.

1822.	Wind.	Barometer.		Thermometer.		Evap.	Rain.	Daniell's hyg. at noon.
		Max.	Min.	Max.	Min.			
th Mon.								
[Aug. 1	W	29·90	29·83	68	51	—	—	
2	N W	30·11	29·83	68	43	—	08	
3	N W	30·11	29·95	68	50	—		
4	N E	29·95	29·93	72	46	—		
5	N	30·09	29·93	74	50	54	—	
6	N W	30·17	30·09	75	50	—		
7	W	30·17	30·08	72	46	—		
8	E	30·08	29·87	79	51	—	—	
9	W	29·88	29·87	76	59	—		
10	N W	29·90	29·88	72	59	56		
11	S W	29·91	29·90	74	56	—	08	
12	S W	29·91	29·87	75	59	—		
13	W	29·98	29·87	74	56	—	03	
14	N W	29·98	29·81	72	55	48		
15	N W	30·14	29·81	72	46	—		
16	W	30·28	30·14	75	49	—		
17	N W	30·28	30·26	82	58	46		
18	N W	30·26	30·22	80	55	—		
19	E	30·22	30·20	78	53	—		
20	E	30·20	30·08	78	54	34		
21	E	30·08	29·98	84	62	—	02	
22	N W	30·03	29·93	84	57	—		
23	N W	30·03	29·98	73	50	41		
24	S W	29·98	29·80	70	50	—	73	
25	W	29·81	29·79	70	47	—	01	
26	N W	29·79	29·78	70	48	32	—	
27	S W	29·81	29·77	71	50	—	21	
28	S E	29·77	29·59	68	54	—	06	
29	W	29·87	29·59	67	50	—		
30	W	30·00	29·87	68	44	—	17	
31	W	30·19	30·00	69	41	42		
		30·28	29·59	84	41	3·53	1·39	

The observations in each line of the table apply to a period of twenty-four hours, ginning at 9 A. M. on the day indicated in the first column. A dash denotes that e result is included in the next following observation.

REMARKS.

Eighth Month.—1—5. Fine. 6, 7, 8. Cloudy and fine. 9, 10, 11. Fine. 12. Cloudy morning: fine afternoon. 13. Drizzling morning: very fine afternoon. 14—23. Fine. 24. Fine day: night rainy. 25. Fine. 26. Showers. 27. A thunder storm about noon: showery. 28. Showery. 29. Fine. 30. Fine: showers in the evening. 31. Fine.

RESULTS.

Winds: N, 1; NE, 1; E, 4; SE, 1; SW, 4; W, 9; NW, 11.

Barometer:	Mean height	
	For the month	29·773 inches.
	For the lunar period, ending the 10th	29·846
	For 14 days, ending the 5th (moon south)	29·818
	For 14 days, ending the 18th (moon north)	30·023
Thermometer:	Mean height	
	For the month	62·532°
	For the lunar period	63·183
	For 31 days, the sun in Leo	63·806
Evaporation		3·53 in.
Rain		1·39

Laboratory, Stratford, Ninth Month, 23, 1822. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

NOVEMBER, 1822.

ARTICLE I.

Sketch of the Geology of Snowdon, and the surrounding Country.

By W. Phillips, FLS. MGS. and S. Woods, MGS.

THE structure of North Wales, as far as our knowledge xtends, has hitherto but in a slight degree excited the curiosity, r exercised the judgment of any person conversant with geoloical inquiries. It has been assumed, however, that in North Vales the vallies are occupied by clay-slate, and the mountains r their summits by greenstone. The discovery of organic npressions near the summit of Snowdon created doubts which ere not easily solved; and while some affected to dispute the ccuracy of the observers, others accounted for it by gratuitous ıppositions. We believe that no part of this country can be alled primitive, and that neither greenstone, clay-slate, nor greyacké, is any where to be found in the district of Snowdonia: this term we comprehend the mountain range bounded on the ast by the Vale of Conwy, extending a line southward to Festiiog: on the north by the Bay of Beaumaris: on the west by e Menai Strait carrying on a line from Carnarvon to Pwlhellie, d on the south, from thence along the coast of Cardigan Bay, rough the Vale of Festiniog.

In our opinion this district offers a new and highly interesting eld of investigation to the geologist. The partial examination its various rocks, and especially of their extremes, would evitably lead to the conclusion that they are of different formions, and possess distinct characters: a more extensive survey fords satisfactory evidence that most, if not all, of them, gradually and imperceptibly pass into each other; that hence they maintain no uniform or characteristic difference, and that all their numerous varieties are interstratified with slates composed of the same materials, and consequently that they are all associated in close family alliance, and owe their existence to a common origin. There are no indications of any one of these slates or rocks being superior or inferior to the rest, and although some of these rocks might be pronounced to be greenstone by a casual observation of them, yet the occurrence of the same organic impressions near the summit of Snowdon and in several other places clearly indicate them to be members of one and the same series. The greater proportion of these rocks may be found within a short compass from Capel Curig, and the situations in which from our own limited experience we should recommend as the most instructive to observers are, the section of the road near Pont y Cyffin; both the new and old road towards Bangor, and beyond their junction from the excavated pass at Ben Glog to a mile or two beyond the inn at Tan y Maes towards Bangor; but above all the mountain close behind the inn called Moel Shabod from the base to the summit on every side. We had to regret that a continuance of unfavourable weather obstructed our visits to the Glyder and the Trefan, but from the information of our intelligent friend Mr. Dawson, of Bangor, who has accurately surveyed the whole country, and a comparison of his specimens, we have every reason to believe they would have furnished similar results. We pretend only to give a hasty and imperfect sketch; limited time and abundant rain prevented us from attempting more, and we proceed to state in detail the evidence we have procured in the hope of stimulating the lovers of the science to investigations still more minute and satisfactory, which will in that delightful region carry with them their own reward.

The general character of the rocks of Snowdonia, and which extend even into South Wales, is of a nature that we did not at all anticipate. Such as possess little or no appearance of a slaty texture, and these are often porphyritic, have for their base a substance greatly resembling steatite or potstone,* often so soft as to yield readily to the knife, sometimes even to the nail, but occasionally so intermixed with siliceous matter, as scarcely to receive any impression from the knife: in the latter case it is generally of a greyish colour; in the former greenish, the colouring matter being in our estimation chlorite, which often is so arranged as to impart to the rock a slaty structure; and we possess the most unquestionable proofs that the impressions of shells occurring within 10 feet of the very summit of Snowdon are in a rock of this nature, of which doubtless the occasional fineness of the grain, and its generally slaty structure, has given rise to the notion that these impressions occur in a greywacké slate. The imbedded substances, when the rock possesses a porphyritic aspect, appear to be contemporaneous nodules harder than the rock itself, quartz, more rarely felspar, frequently carbonate of lime, the three latter being generally crystalline, transparent, and very minute; chlorite, however, is so commonly an ingredient of all the steatitic rocks, either in very minute particles, or in layers, that it may be said to form almost an essential ingredient, and is commonly present in so large a proportion as to impart its greenish colour to the mass.

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* Some of the rocks of which this substance forms the base, or which are constituted of it, possess so nearly the characters of some varieties of steatite or potstone, as to prevent all hesitation in pronouncing them to be allied to those substances, while others assume a talcose character; their connexion with chlorite (which almost always accompanies them) seems to authorize the conclusion that they are all of one family, since the greater number of these minerals analyzed by Klaproth, Vauquelin, and Lampadius, were found to consist of the same elements; namely, silex, alumine, with 20 to 30 per cent. of magnesia, and a small proportion of lime. Hitherto, however, we have been speaking of the resemblance of the rocks of Wales to steatite or potstone, only from their external characters, and judging by these alone, we repeat that no hesitation would be felt in considering them as varieties of those substances. Being anxious, however, to ascertain by a reference to chemical agency, how far the chemical characters agree with the external, we placed in the hands of Mr. Richard Phillips five specimens, varying considerably in aspect, but all allied by interstratification. These were found to

Chlorite, however, sometimes prevails so greatly, as almost to exclude the other ingredients, and then appears in the form of chlorite slate, which occurs near the summit of Snowdon, within perhaps 20 feet of the steatitic rock containing the impressions of shells; and is even interstratified with layers of the same nature.

The two extremes, therefore, appear to us to be steatite and chlorite; but each of these two substances is often so modified by combining with the other in different proportions, and probably also by the intimate dispersion through the mass of siliceous matter, and by its occasionally imbedding small nodules of calcareous spar, felspar, and grains of quartz (the latter in one instance prevailing to the almost total exclusion of all the rest), that the rocks assume a great variety of aspect, and even appear to differ so greatly, that nothing short of an inspection of the whole series, or seeing them either interstratified or passing into each other, as we have mostly seen them, would suffice to produce a conviction of their actual and even intimate connexion.

The slates forming so considerable a proportion of the surface of this country, are also of very different aspects; varying from nearly pure steatite of a greenish colour, soft enough to yield to the pressure of the nail, through still harder varieties, both externally and by transmitted light of the same colour, to

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consist chiefly of silex and alumine, but included a small proportion of lime. On a repetition of his experiments for the purpose of ascertaining whether an alkali is or is not present in a rock most nearly resembling steatite, no alkali was detected, but the presence of a very minute trace of magnesia was indicated. These rocks, therefore, differ from steatite and potstone in containing little or no magnesia; but as it is essential to adopt some name for the sake of reference in the following pages, we shall in speaking of them use the term steatite, which may serve until some more appropriate designation shall be given to them.

the blue and purple varieties of ordinary slate. These all occur interstratified with the rocks above-mentioned, and certainly partake of their nature. The slates of the vast quarries at Nant-francon dip beneath these rocks towards the south east, while similar rocks occur at the coast on the north-west of Bangor; on these rocks we incline to believe the slates of Nant-francon actually rest; but having had no opportunity of ascertaining the fact, we recommend its investigation to future observers. Of this, however, we are assured, that we have often perceived similar slates interstratified with the same rocks, and conclude from this and other circumstances, that the slates of that quarry strongly partake of the nature of chlorite slate.

One fact, and we consider it as a somewhat remarkable one, is, that the plane of the cleavage of the slates and slaty rocks runs everywhere (with the exception of one hill) from the east of north to the west of south, the slates being in some few instances vertical, but more commonly dipping at a high angle towards the west of north, or east of south. From this circumstance, it may, perhaps, be argued with much probability, that even the most mountainous tracts of North Wales do not present any appearance of that kind of disturbance, which, in some countries, is not uncommon, and which is considered to have arisen from depression on the one hand, or elevation on the other. The slaty cleavage being, as we have already described it, it follows of course that no mantle-shaped masses were observed.

We are not disposed to view the circumstance of the almost uniform direction of the plane of the slaty cleavage as an isolated fact, relating simply to the geological structure of Wales, but as being probably connected with that of our island generally; for it is well known that the newer strata of England possess the same general bearing, as may be perceived at once by casting the eye over a geological map of our country. When thus viewed in connexion with a series of numerous beds, this point appears to assume an interest at once both important and extensive.

Shells occur in greater abundance, and of more varieties, than we expected to find.

In North Wales, we did not perceive a single instance of contortion either in its rocks or slates.

With the intention of ascending Snowdon by all the customary routes, our first station was at Capel Curig Inn, but for several days our hopes of making an ascent were disappointed by the wetness and haziness of the weather, which prevented us from even discerning the mountain, and ultimately deprived us of the opportunity of ascending it from all the various points. Meantime we were induced by Mr. Dawson, who did us the favour of a visit at Capel Curig, to ascend and examine the neighbouring mountain Moel Shabod. Previously to our ascent, the gentleman above named mentioned to us one circumstance which forcibly attracted attention; namely, that the slate quarries of North Wales extend along a line bearing north-east and south-west from Aber to near the Pwlhelli (a fact we had no personal opportunity to verify), and this information first drew our peculiar attention to the direction of the slaty cleavage, and which we rarely or never failed to notice afterwards.

Moel Shabod, whose base descends to the Lake immediately below the Inn, may be described as a single mountain rising to the height of 2800 feet above the level of the sea. Its north-western foot may be said to touch the base of Snowdon beneath the pass of Llanberris.

Our first ascent was up the northern side of the mountain, to its summit from the back of the Inn at Capel Curig, and in general terms, it may be said, that three rocks, at first sight differing considerably from each other, presented themselves to our notice in succession, each prevailing at different elevations. The lower third, or the base, as it may be termed, consisted chiefly of a rock which is often so slightly granular that at first view it appears homogeneous: the middle region appeared to consist principally of slates, and the upper part of a rock which sometimes has greatly the appearance of a greenstone, but manifestly partakes of the nature of those at the base.

A close inspection of the prevailing rocks of the lower third, or base, of the mountain, however, discovers, when assisted by the glass, that they are composed of a translucent substance, enveloping very minute portions of a green matter, imparting to the grey colour of the rock a greenish tint, which is heightened to green by the addition of moisture: when reduced to small thin fragments, the transmitted light is green: in a large proportion of our specimens, no quartz nor any other substance appears to be imbedded, nor does any effervescence take place on exposing them to the action of muriatic acid: in others, however, of a larger grain, some effervescence occurs: in others again, small transparent crystalline particles are imbedded, which do not yield either to the knife or acid, and which we, therefore, consider to be felspar, being of a more lamellar structure than quartz; the lower rock often contains imbedded masses of a much closer grain, and harder than the rock itself. These rocks yield easily to the knife, passing before it into a greyish powder, and their fracture is uneven and splintery: we consider them to consist of a species of steatite enclosing chlorite, and we are so inclined, not simply from their general aspect, but also from the nature of one rock with which they are associated near the base of the mountain, and opposite to the back of Capel Curig Inn. It perfectly resembles a steatite of a greenish or yellowish-green colour, probably from the more intimate dissemination of the chlorite through its mass; for chlorite does not, as in the former rocks, appear in minute particles, as its partly weathered edges still incline to green, though the absolute surface is a greyish-white, and its structure is in some degree slaty. It contains, besides felspar, masses of a white substance, which in the internal part of the rock are translucent, when weathered white and opaque; these have much the aspect of hornstone, and often inclose crystals of quartz in their cavities; they scarcely yield to the knife; they vary in size from half an inch to six or seven inches in diameter; and they appear to us to consist of the same material as the rock itself, but include a far greater proportion of siliceous matter, and we believe them to be of contemporaneous origin with the rock inclosing them.

The slates of the middle region of the mountain occasionally do not differ in their external characters from ordinary slate, but by transmitted light through the thin edges, they often, though not always, appear of a green colour. These slates, as well as the rocks, frequently contain small cubic crystals of iron pyrites.

The rocks of the upper third all partake largely of the aspect of steatite, which, though it may be said to form the imbedding substance or paste, has often a crystalline aspect, and is always porphyritic from its containing distinct macled crystals of transparent felspar, opaque calcareous spar which effervesces with muriatic acid, and prismatic crystals of a blackish-green augite, affording cleavages sufficiently brilliant for the use of the reflective goniometer; the rock is generally of a greenish colour, which is much heightened by the addition of moisture, and the examination of it in that state discovers the presence of a multitude of minute particles (probably of chlorite) of a much brighter green than the augite, and to which the green colour of the rock may be attributed. These rocks are of considerable hardness, often very hard, and they suffer so little from exposure, that the weathering has scarcely proceeded to the depth of one-fourth of an inch into the solid rock, and we could easily distinguish this upper rock by a peculiar external roughness. It returns a ringing sound, when struck by the hammer.

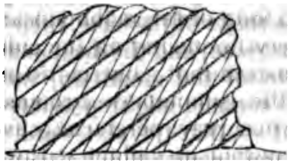
But although the rocks abovementioned prevailed each at different elevations, further investigation proved that the slates of the middle, and the steatitic rocks of the lower region, are frequently interstratified: this was observed in very many instances in crossing the eastern side of the mountain at a considerable elevation, and again very frequently on the side of the road near Pont y Cyffin at the eastern foot; but in no instance did we observe the rocks of the summit so circumstanced. Near Pont y Cyffin likewise we clearly perceived several instances in which the same rock as that prevailing at the base of Moel Shabod, passes into a greenish slate, which is soft and granular.

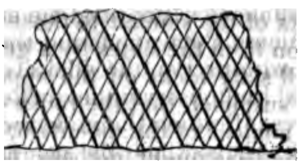
But it must be remarked that this interstratification of the slates with the rocks of the base invariably takes place parallel with the ordinary cleavage of the slates, and wherever this appearance is observable, it is always in the same direction; the cleavage of the slates ranging, as has already been noted, uniformly on this mountain, and in every place we visited (one instance only excepted) east of north, and south of west.

The dip of the slates, however, on this mountain is by no means regular: in the upper part of their range on the northern side, they are nearly vertical, while on the southern side, they dip at a considerably low angle towards the north-west; but in the middle region towards the south-east, at an angle of 54°.

The slates, as well as the rocks of the lower region, both where they are interstratified, and where they appear separately, have a remarkable tendency to assume the form of steps, or terraces, which is considered to be characteristic of rocks belonging to the comprehensive trap formation, as well as in another particular, namely, in assuming a semi-globular form, which does not seem to be the consequence of atmospherical action: this appearance was observable in very numerous instances on this mountain, and elsewhere.

There is one circumstance relating to the stratification of the slates and slaty rocks of this mountain which deserves mention, because it involves appearances that would tend to mislead, without sufficient investigation. It is well known that slates have a tendency to separate into a rhombic form; and in several instances on this mountain, the lines at which the slates would so separate were remarkably distinct on masses presenting a surface of 20 or more feet every way. But these lines of apparent, and often of actual separation, were much stronger than those of the slaty cleavage, being continuous quite across the surface of the mass, at about a foot apart, and above an inch in width and depth, assuming, therefore, the appearance of lines of regular stratification. The following sketches show the appearances observed on two neighbouring surfaces, of which the lightest lines, representing the slaty cleavage, are in the same direction in both; while the lines of apparent stratification are in opposite directions, both nevertheless tending to produce the rhombic form.





The extremely unfavourable state of the weather did not permit that nice examination of the rocks of this mountain, which alone would have enabled us to detect any vestiges of organic *remains, if any exist in them.* In the garden wall of the Inn, we found in a mass of rock perfectly resembling some of the varieties plentifully distributed at its base, the impression of a shell, much resembling those which have been so abundantly discovered in what hitherto has been termed greywacké, near the summit of Snowdon.

In the loose masses lying beside the road near Pont y Cyffin, we found several shells, some resembling those just mentioned; others differing from them. These masses had apparently been cut from the rocks on the side of the road; some of them were slaty; others were not, but perfectly resembled the base rocks of Moel Shabod, from which their locality was separated only by the road and the river. The slate is occasionally hard enough to scratch glass, is translucent on the edges, and green by transmitted light, but externally has the character of ordinary slate. In one of the stones used in the construction of "the tap," belonging to Capel Curig Inn, near the door, we observed a considerable mass containing the impressions of several shells.

The sides of Moel Shabod are in so considerable a degree covered by herbage,—by grass, by fern, and here and there by heath,—as on a very large proportion of its surface to hide very completely the rocks from observation: this, we cannot doubt, has arisen from the decomposition chiefly of the slates prevalent in the middle region of the mountain. On the northern side of it (we had not an opportunity of observing the southern side at about the same height), there appears a sort of terrace covered by long grass, hiding from view the summit of the slates, and between them and the porphyritic rocks constituting the upper part of the mountain. Above this terrace, we found no appearance of herbage, nor any slaty rock; the whole consisting of loose blocks of the rock already described as constituting the upper third part of the mountain. These blocks are of very considerable size generally, lying without regularity, and they had no appearance of the columnar form, except, perhaps, in a very few instances; but on a considerable part of what may be termed the long line of its summit, which is often less than 20 feet in breadth, large and apparently columnar masses were ranged side by side in a horizontal position, across the ridge in north and south direction, and always presenting an edge uppermost.

The seeming difference in the mineralogical characters of the rocks of the summit to those of the rocks of the base, their separation from the slates by a grassy terrace, and the disappearance of all slaty rocks on the upper third of the mountain, induced the suspicion that the rocks of the summit might possibly form the nucleus of the mountain, and if so, that the slates and rocks of the base might, perhaps, have been deposited upon them. If this were the case, analogy led us to expect that a careful examination of the southern side of the mountain, which is far more precipitous, and, therefore, in its upper part less covered by herbage than its northern side, would expose the slates and lower rocks in mantle-shaped masses; we did not, however, succeed in realizing this expectation, for on both sides, the cleavage of the slate is in the direction of north-east and south-west, being on the northern side nearly or quite vertical in their nearest apparent place to the summit, while on the southern side, the dip is towards the mountain itself at a considerably low angle. There were, however, some circumstances which tended to confirm, though they did not fully satisfy us, of the correctness of the notion, that the rocks of the summit constitute the nucleus of the mountain. On the south-east side, there is a small lake, called Llyn y Foel, perhaps 1000 feet below the summit of the mountain. About 300 feet beneath this Llyn, we observed a rock much resembling that of the summit, as it were, protruding from the side of the mountain, but did not perceive the mode of connexion between it and the slates on each side and above it. Again, above the Llyn, we found the same rock, dipping down in continuity, from the summit, and also apparently beneath a vast mass of slates, their actual contact being covered only by about three feet of alluvial matter. Again, having crossed the dip of this apparent buttress of the summit rock, above Llyn y Foel, we descended over slates and slaty rocks perfectly resembling those already described, the slaty cleavage being constantly north-east and south-west; but in our long walk in returning to Capel Curig down the slant of the mountain, we perceived what we supposed to be still another buttress of the summit rock, covered on both sides by slates. Notwithstanding these repeated appearances of this rock in the manner above described, it would require a much longer time than we could devote to the investigation to ascertain whether or not the nucleus of the mountain be of the rock appearing on the summit, and we wish to recommend this examination to future observers.

It may be mentioned, that in traversing the long and narrow ridge which constitutes the summit of Moel Shabod, we perceived two chasms, at a little distance apart, from 10 to 20 feet wide each, and traversing the summit in nearly a north and south direction. The continuation of these chasms was soon lost beneath the grass covering to a great height the more easy slope on the northern side, but they were perceived to a great depth down its precipitous side on the south. They descend nearly to Llyn y Foel, though in great degree filled by ruin and alluvial matter, as we could perceive in looking upwards from the banks of that Lake. These chasms appeared to be the consequence merely of the separation of the rocks. The rubbish with which they were filled at the summit appeared on examination to consist altogether of fragments of the adjacent rocks, which, however, might possibly hide the substance of which they may be filled (if indeed they be dykes), except where it has been decomposed by the atmosphere, and carried off by the rains.

Among the loose masses on the mountain, and immediately around it, we found several varieties of rock which deserve mention.

1. Considerable masses of a rock which is extremely common; it is very hard, cuts glass easily, and yields with difficulty to the knife; breaks with a flat conchoidal fracture, and has much the aspect of flinty slate, but is frequently porphyritic from small imbedded masses of opaque calcareous spar, and transparent crystals of felspar. By the assistance of a glass, however, this rock appears to have a somewhat granular texture, and a waxy lustre, and greatly resembles those of the masses inclosed in the rocks at the foot of the mountain, and which we conceive to be of contemporaneous formation with the rock itself. The colour has a tinge of green, derived, as we conclude, from chlorite, which appears arranged in irregular, though in some degree parallel lines throughout the mass, and are completely green by transmitted light. Other varieties do not yield to the knife. On the mountain, we found a loose mass consisting of crystallized quartz, opaque felspar (?), and chlorite in small quantity; and near the foot of it a rock, not differing from those of the base inclosing small ovate or rather elliptical masses, of about the same appearance and hardness as the rock itself, arranged in one direction through the rock. In the wall of the Inn garden at Capel Curig, a fragment of considerable size of a rock which consisted of somewhat round masses varying in size from a pea to that of a cricket ball, of a substance which appears homogeneous, yields with difficulty to the knife, and has the aspect of some of the closer grained varieties of the base rock: these were cemented, and coated by a substance, which is soft, slaty, glistening, and somewhat unctuous to the touch, and which is manifestly talcose. In a field on the other side of the road to that on which "the tap" of the Inn stands, a very large mass of what may properly be termed a puddingstone, consisting of nearly spherical masses about the size of a nut, of crystalline quartz, connected sometimes with calcareous spar, in other instances containing a substance that appears to be the hydrous oxide of iron; these masses are imbedded in great quantities in a paste, not to be distinguished from some of the base rocks, but very much harder, probably from the mechanical diffusion of silex throughout the mass.

The continuance of rain, and the mist which unceasingly enveloped Snowdon, rendering it still nearly impracticable to ascend that mountain, we ventured to set out with the intention of seeing the rocks of the Pass of Llanberris forming one of the grand separations of Snowdon from the neighbouring mountains. In this, we were only partially successful, as the rain continued the whole of the time we were in the Pass, and sometimes fell very heavily; we did not, therefore, reach Llanberris.

Having been informed by Mr. Dawson that he had discovered a trap dyke on the very summit of the Pass, and that we could not descend without treading upon it, we sought for it with much attention, and repeated our search upon a subsequent visit, but in vain. The summit appeared to us to consist chiefly of alluvial matter, among which we found, and especially in the horse road, several specimens of a rock much resembling decomposing greenstone or trap, not very uncommon upon Moel Shabod, and of a similar character to the decomposing rock on Cader Idris, which has given rise to the notion that volcanic debris are discoverable on the latter mountain: the cellular appearance of the rock is derived from the decomposition of the particles of calcareous matter which once filled its cavities, while the blackness of the mass arises from the altered state of the iron included in the chlorite, forming an integral part, or the colouring matter of the rock itself.

The sides of the pass, as its name denotes, rise to a great height on both sides; but they afforded us, so far as we were able to inspect them, no variety differing in any important degree from the slates and rocks of what we have termed the base of Moel Shabod, and the slaty cleavage runs in the same direction; viz. north-east and south-west. The same rocks, as far as we were competent to judge of them from below, or from the numerous and often very large fallen masses, were the same to the very summit, with the exception of a few varieties, which we shall presently notice. We perceived many instances on the large scale of their taking the rounded form already noticed. These separate masses are often traversed by nearly horizontal lines, or by small cavities which, from their nearly ovate figures, might be assumed to have been derived from their having once been occupied by substances of similar form: they do not, however commonly present any impression; but the surface of several fallen fragments of considerable dimension that had separated along these lines, were covered by cavities altogether resembling each other, and several of them presented on one mass the casts of ribbed shells. These lines of cavities, it will be observed, being nearly horizontal and parallel to each other, were about, but not quite, at right angles to the slaty cleavage of the rock. We succeeded in detaching the impression of a shell lying in one of these nearly horizontal lines, from a rock perfectly resembling that variety, at the base of Moel Shabod, which has most the appearance of homogeniety.

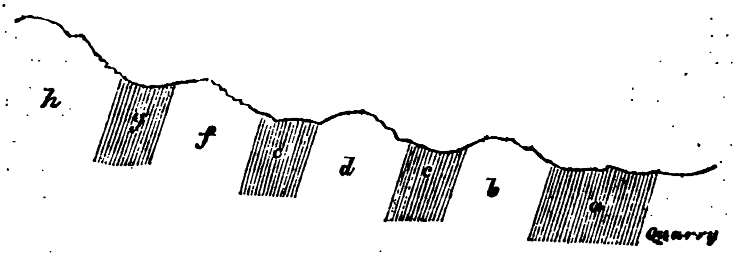
Among the fallen masses in this pass, we found some resembling flinty slate, and possessing the same character, though harder than those occurring around and upon Moel Shabod. We also found masses greatly resembling a steatitic substance, of a yellowish-white colour, and so hard as to yield with difficulty to the knife, though here and there yielding to the action of acid; also other varieties more decidedly slaty, of a yellowish or greenish colour, and softer. The following varieties of rock were also found in the pass. Masses resembling in every respect the base rock of Moel Shabod, inclosing others of a darker colour, which also were softer, and very decidedly steatitic. Very large masses of the same rock, often highly steatitic, enclosing flinty slate, in pieces from the size of a pea to that of the human head. A steatitic rock composed of small particles of various colours, forming in the aggregate a paste of a greenish hue, enclosing (apparently) angular pieces of flinty slate. Red and white hornstone enclosing translucent quartz and steatite. It could be perceived only on the weathered surfaces of many of these rocks that their structure was slaty.

Our curiosity was naturally directed to ascertain whether the large slate quarries of Nant-francon constitute a part of the general series or not. These quarries are situated near the foot of the northern termination of a mountain, a little on the west of the road from Capel Curig to Bangor, about two miles beyond the Inn at Tyn y Maes. They consist of three principal cavities, averaging, perhaps, 200 to 300 feet in depth, by as many in length and width. About 950 men and boys are employed in quarrying, splitting, and trimming the slates. The best are not blasted, but split by wedges parallel to their cleavage plane; and thus thick masses are obtained, which again are split by means of a mallet, and the application of the edge of a broad and sharp chisel parallel to the laminæ. The slates are of two principal colours; viz. of the ordinary slate colour, and of a purplish or reddish hue: the latter are considered as the best slates, being of a finer grain, and splitting readily into thinner laminæ than the others: occasionally there appear specks or large patches of a green colour, also having a slaty fracture, but of a closer grain, very much harder, and translucent on the edges. The quality of the slate often varies greatly in a few yards, or even feet.

In what degree the slates of this quarry partake of the nature of chlorite, it is, perhaps, impossible to determine; it is very certain, however, that the irregular surfaces produced by fracturing the slate in a direction opposite to its cleavage present a granular aspect and glimmering lustre much resembling chlorite, and that a considerable but very irregular vein of quartz and chlorite accompanied by calcareous spar, traverses the slates, the chlorite being sometimes in a crystallized state, sometimes in the form of slaty chlorite; large masses of the latter are observable on the south-eastern part of the quarry, and those blocks of it were very numerous, as were also rolled masses of it in several parts of the works.

The cleavage plane of the slates here, as every where else, is in the direction of north-east and south-west, and dips to the south-east at about 68°. On the sides of the quarry were perceived numerous fissures possessing a certain degree of parallelism among each other, though wanting completely that evenness and continuity which belong, generally speaking, to the lines of regular stratification; and as these fissures are nearly horizontal, they serve materially to assist the quarry-man in his operations: fissures in a downward or almost vertical direction nearly at right angles to the cleavage plane, are less common, but experience has proved that the slates in mass may be split with considerable ease in this direction, though without producing an even surface; thus reducing them into fragments partaking more or less of the rhombic form common to slates after long exposure to the action of the atmosphere.

As the slates of this quarry dip uniformly *towards* or *under* the northern termination of the mountain at the foot of which it is situated, we became desirous to ascertain the nature of the rocks constituting the high ridges crossing in the direction of north-east and south-west, the higher parts of the acclivity, and which of course are incumbent on the slates. Ascending, therefore, we cautiously examined every rock *in situ* to a considerable height. For some distance we found only a purplish slate; but the following sketch will serve to explain what we perceived:



a. Continuation of the quarry slates, but less firm, and of a purplish colour.

b. A granular rock, yet somewhat slaty, resembling the base rock of Moel Shabod, and effervescing by the application of acid.

c. Green slates, resembling chlorite slate.

d. A green granular rock, containing round masses of calcareous spar, decomposing on the surface.

e. Slates.

f. A rock chiefly resembling *d*, but containing abundance of crystallized quartz, and occasionally chlorite: effervesces in patches; but in some places appears to consist chiefly of steatite.

g. Very hard slates of a greenish colour.

h. Rocks resembling *f* and *d.*

Most of the rocks and slates of the preceding sketch greatly resemble those of Moel Shabod, that we cannot hesitate in referring them to the same era, and here also the direction of stratification is in the plane of the slaty structure.

In our walk to the Inn at Tyn y Maes, we examined the rocks which have been cut through in forming the new road to Bangor nearly at right angles to the plane of stratification; but we observed only the repetition of the rocks already noticed, alternating, perhaps, 10 or 12 times with slates, differing little or nothing from the varieties already described.

At Ben Glog, which is nearer to Capel Curig than the Inn at Tyn y Maes by about two miles, we found a considerable change in the appearance of some of the rocks. The prevailing character here is so decidedly steatitic, that the rock often assumes the appearance of a slaty steatite, or of a talcose rock of a green colour, soft enough to yield easily to the nail on the cross fracture, and coated on the plane of its regular cleavage by a soft shining substance which appears to be green talc; occasionally, however, it is harder, splitting readily into thin laminæ, the edges of which cut glass readily, though the plane surfaces are soft; we conceive this, therefore, to consist of alternate and very thin layers of talc and quartz. Specks of calcareous spar appear in some places, and here and there also macled crystals of transparent felspar. Another rock with which the above are associated appears to consist of greyish steatite, which is very hard, owing as we assume to distribution of silex through the mass. This rock has been cut through close to the bridge of Ben Glog, exposing its enclosed masses often of some inches in diameter, of crystalline calcareous spar, of which the crevices are filled by transparent quartz: but as some of these rocks effervesce on the application of acid to the surface, the carbonate of lime appears in these instances to be generally disseminated throughout the mass. A rock also occurs here plentifully, which is finely granular, of a bluish-grey colour, and extremely soft, which, by exposure, loses its colour, and passes into a sort of yellowish slaty steatite, which is very hard, and perfectly resembles the steatite already noticed as having been found among the debris of Llanberris Pass. It is said to be among the slaty talcose rock above described that the honestone is found and quarried near this place. These slates and rocks are interstratified in the usual direction of north-east and south-west.

On the low walls of Ben Glog Bridge, we found several masses of slate, and of rock having more or less of slaty structure, containing the impressions and casts of shells.

In the walls bounding the New Road between Ben Glog and Capel Curig, we observed very numerous tabular masses of a rock having much the aspect of a flinty slate, and perfectly resembling the loose fragments already described as occurring around Moel Shabod, and in Llanberris Pass. These tabular masses we cannot doubt from their form were found interstratified with the other rocks of this neighbourhood, and from the trifling appearance of disintegration on many of them, we suspected them to have been quarried for the purpose to which they have been applied. We did not, however, succeed in finding the quarry, owing, perhaps, to the still very unfavourable state of the weather. We have since been informed by Mr. Joe. Woods, that he has seen the quarries in question in that neighbourhood, and that these masses are found as we had suspected. Rolled masses of this rock, both porphyritic and otherwise, prevail every where in the low ground and valleys, and have been much used as a building stone.

On our return to Capel Curig, we passed the foot of Trefan, a mountain of considerable elevation, whose remarkably serrated outline, and rugged scarp attracted our attention, and induced the wish of ascending it for the purpose of becoming acquainted with the nature of its constituent rocks. But the extreme wetness of the weather deprived us of the power of seeing much that would have been highly interesting. Mr. Dawson afterwards presented us with a specimen of a rock having the appearance of a conglomerate, and resembling some already described as appearing near the base of Moel Shabod, and which he informed us prevailed on the ascent of Trefan. The same gentleman showed us a small fragment of one of the two remarkable upright pillars on its summit, which resembled some of the small grained rocks, hereafter to be noticed, as occurring near the summit of Snowdon.

(*To be continued.*)

ARTICLE II.

On Siliceous Petrifactions imbedded in Calcareous Rock. By the Rev. J. J. Conybeare.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Bath Easton, Oct. 2, 1822.*

THE perusal of the note occurring at p. xii. of the preface to the "Geology of England and Wales," has induced me to submit to a rude examination such specimens as I possess of siliceous petrifactions imbedded in calcareous rock. I wish that my results were more calculated to throw light upon their production, and that they may induce some one better qualified to pay the subject that attention which it demands.

1. *Coral in separate Branches protruding from the Surface of Masses (apparently either Nodules or weathered Portions) of dark blue Limestone (Mendip Hills).*—The exposed portion of coral

entirely siliceous, and in parts studded with minute crystallizations of quartz. On exposing the mass to the action of diluted muriatic acid, I found that the siliceous matter of the coral penetrated but a very little way into the limestone; even at the point of entrance the branch became smaller, and its organic character much obliterated.

2. *Siliceous Petrifactions of the Stems and Plates of the Encrinite beautifully preserved in Relief on the (weathered?) Surface of Limestone Blocks. (Barrington Cleve, and Mendips.)*—On exposure to acid exhibited the same phænomena with the preceding. In both cases even the smallest nodules or plates of limestone exhibit in their interior, organic portions, of the same species with those silicified on their surfaces, but preserved as usual in calcareous matter. A lucky fracture, even while I am employed in transcribing my notes, has denuded for me a branch of coral, which is converted on the surface into silex, but almost from the point of entrance is *continued* in calcareous spar. The limestone in which these fossils are imbedded gave, upon solution in a dilute acid, a residuum of from six to seven per cent. consisting chiefly of silex in a state of minute division. It struck me forcibly that these and all specimens of the same nature, which I recollect to have seen from the mountain limestone, appear either to have been detached from the exterior of the strata, or to have occurred in small insulated masses. This, joined to the circumstance already noticed of the change which takes place in the substance of the fossil after its entrance into the limestone, induces a conjecture that the source of the silex is to be sought for in the iron shot marle which occasionally fills up the interstices between the calcareous strata. On the causes which may have operated to produce, either in this or any other formation of limestone, the alternation of marly or slaty beds, I do not venture to speculate.

3. *Gryphites preserved in coarse Chalcedony, from Dunraven.*—These are found both in the solid lias and in the marle which alternates with it. A portion of the former, in which a chalcedonic shell was totally enveloped, afforded with acid a residuum of silica mixed with a small proportion of alumina, iron, and bituminous matter. The whole residuum amounted to nearly 15 per cent. of which two-thirds at least must have been silica. In the lias which furnished these specimens, I observed large cornua ammonis having their interior partly studded with crystallized quartz. The composition of the lias beds, especially towards their exterior, is so variable that other specimens might possibly furnish a yet greater proportion of silex.

4. *A Specimen of Chalcedonic Shell imbedded in a very Chalky Form of Green Sand (from near Stourhead, Wilts).*—From the nature of the matrix, the action of diluted acid very readily extricated the shell (a portion of a large pecten.) The acid then attacked the shell itself, and before its action ceased, had dis-

solved a considerable portion of it, a fine siliceous powder falling during the process. The undestroyed portion of the shell remained as a thin irregularly shaped mass of chalcedony. On this of course acids had no further effect. The specimen, therefore, exhibits a singular instance of a shell preserved partly in siliceous and partly in calcareous matter.

These are the only cases of this phenomenon which I have hitherto had the opportunity of examining.

Believe me, my dear Sir, very truly yours,

J. J. CONYBEARE.

ARTICLE III.

On the Geology of the Malvern Hills. By the Rev. J. J. Conybeare.

(To the Editor of the *Annals of Philosophy.*)

MY DEAR SIR, *Bath Easton, Oct. 4, 1822.*

A SHORT residence at Malvern in the summer of 1821 enabled me to verify most of the statements contained in your brother's very accurate survey of its neighbourhood, and to observe some few circumstances, the detail of which (as they are unnoticed or not viewed in the same light, either by that gentleman, or by Mr. L. Horner), may, perhaps, be regarded as contributing somewhat more towards the history of that interesting tract.

I would first notice the character of the two remarkable conglomerates connected with the syenitic rock.* The first of these occurs in blocks (which I was unable to trace to their original site) a little southward of the road leading to the Wych. (The wall in particular which supports the ground in front of the cottage, named North Lodge, contained many specimens of it.) It is composed of amorphous nodules of the small-grained red syenite, abounding in felspar, imbedded in a paste, so precisely resembling the nodules themselves, as to preclude all supposition of its being a mechanical mixture. A *recomposed* granitic rock (as it has been termed) possessing nearly the same characters, was found by my friend Dr. Daubeny in loose blocks near Ardnamuchan, N. B. M. Bouè (Geologie de l'Ecosse, p. 22), mentions a rock of the same nature as occurring near the Fall of Fyers. (See also Dr. Macculloch's Classification, p. 580, G. 6.) Another conglomerate, evidently belonging to the same class, was found *in situ* on the road leading from Castle Morton Common to Ragstone Hill. In this both the nodules and the cementing mass are of a dark and apparently homogeneous trap,* not much unlike that through which the passage of the Wych is cut. This rock, especially the imbedding paste, seems to decompose with great readiness into a greasy clay,† a character often observable in the less crystalline and obscurer forms of greenstone. The quarry from whence my specimens were obtained exhibited no traces of stratification. Both these conglomerates occasionally contained nodules of quartz. Whether they are to be considered as contemporaneous with, or posterior to, the formation of the great syenitic mass with which they are connected, I cannot pretend even to conjecture. Appearances certainly occur which might countenance the belief that there has been, to some extent at least, a second infiltration or injection of matter identical with one or other of the constituents of the original rock. Thus in one spot I found a vein of white felspar containing angular fragments of dark green trap. Some other of the felspar veins would, perhaps, be held to be *true* veins, and the dyke observed by Mr. W. Phillips seems to have every pretension to the character of a *true* dyke.

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* These are, if I mistake not, the last rocks mentioned in Mr. W. Phillips's Catalogue. *The former is probably* that described by Mr. L. Horner, T. G. S. vol. i. . 295. § 26.

At the north or rather north-west extremity of Ragstone Hill the greywacke appears in force, and assumes a much more characteristic aspect than in those points where it alternates with the transition lime. Its slaty variety here occurs as a very dark (nearly black) shale, the appearance of which joined with the resemblance which its compact form bears to some of the coal measures, has tempted some of the neighbouring cottagers to make trial for coal. This end of the Hill would, perhaps, be the best spot for studying the character of the greywacke, and its relation to the syenite. I have to regret that neither my time nor my health, would permit my accurately examining, or paying it a second visit.‡

At the old shaft, known as Williams' Mine, I found no trace of metal, and was disposed to acquiesce in the general belief that the adventurers had been misled by the pseudo-metallic aspect of the Cat Dirt (disintegrating mica), until a lady who had examined the *deads* with greater care, informed me, and I afterwards observed myself, that some few portions of the rock contained very minute specks of yellow copper and patches consisting of, or at least coloured by, its carbonate. They were of very rare occurrence, and the whole quantity which I saw could not have exceeded a few grains. They who know the sanguine temper of miners will, however, understand that these must have been quite sufficient indications to ground a *venture* on. Near the debris of this mine the hornblende and epidote may be found crystallized more distinctly, and the dark-green mica aggregated in larger and more characteristic masses than is usual. The latter (the mica) is of that variety which occasionally scratches glass. It fuses readily before the blowpipe into a greenish-black glass. The disintegrating variety (Cat Dirt) is more difficult of fusion, and the result more slaggy. I may add that the quarries of Ledbury have of late produced many specimens of a trilobite nearly resembling that of the South Welch transition rocks of Dynevawr and Built, and that calcareous spar of a very pure rose colour is found in large laminæ interposed between the strata of limestone. I could not find it crystallized. If it occurs in that state, it must form specimens of singular beauty.

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* See Boué, p. 130, for an analogous fact in dolerite (augite rock). On the Connexion of trap and syenite; see Dr. Macculloch on the Island of Rum.

† Is it not this which has been termed by Mr. Horner and Mr. W. Phillips steatite?

‡ Hand specimens are met with in which it seems difficult to ascertain whether they belong to the trap or the greywacke formation. Is it possible that they may graduate into each other?

Believe me, dear Sir, very truly yours,

J. J. Conybeare.

Article IV.

Reply to Messrs. Young and Bird. By N. J. Winch, Esq.

(To the Editor of the *Annals of Philosophy.*)

Sir, *Newcastle-upon-Tyne, Oct. 7, 1822.*

However unwilling I may be to occupy your pages with controversy, yet as an honorary member of the Geological Society of London, I consider it a duty I owe to that body not to allow the misrepresentations of the Rev. Mr. Young and Mr. Bird, of Whitby, printed in your last number, to stand on record unanswered, particularly as the Society is mentioned in their letter. The hills and a portion of the flat country in the north-east of Yorkshire are now ascertained to belong to the lias formation; the upper beds consist of sandstone, shale, and limestone, with thin seams of coal, the lower bed of shale or alum slate; but the limestone and coal are not interstratified with the other beds throughout the whole district, and in many places the alum shale bassets out, or possesses a very thin covering, *my critics' "ideas of geology being formed only from the inspection of a very limited district."* (See Introduction to Conybeare and Phillips's Outlines of Geology, p. xlv.) They have considered the upper strata and lower stratum as distinct formations, and upon this assumption the whole of their statements rest. On such false premises they attempt to convict me of what they term gross mistakes, but let my account of the outline of the lias be compared with Mr. Greenough's map, and the discrepance between me and that gentleman will be found to be very inconsiderable, especially when it is recollected that six years have elapsed since my short tract was written. With their own hypothesis in view, your correspondents assert that I describe the oolite formation as immediately succeeding or being in the vicinity of the northern Cleaveland Hills, but my line of demarcation, already referred to, will serve to show the fallacy of this statement; and Robin Hood's Bay not being more than between six and seven miles from the oolite hills, I still think these may be seen when three miles south of that place. Another of their leading remarks is, that I associate Danby Beacon with the mountains of the northern part of the district, " whereas it is one of the most southern." The truth is, that the hill in question is 26 miles distant from the southern, and eight from the northern termination of the lias formation at the foot of Rosebury.

The minor objections may be dismissed in a few words. Felay Head is in the oolite district (see Greenough's map). All the shale beds of the lias formation contain nodules called ironstone balls, consisting of carbonate of lime, clay, and peroxide of iron. That the hard shell limestone of the vale of Pickering is distinct from the Thirkleby limestone, I am now convinced by the evidence of Prof. Buckland in his paper on the Kirkdale cave, the former being a bed in the oolite, and the latter a bed in the lias formations. I am, Sir, your obedient servant,

N. J. WINCH.

ARTICLE V.

On some peculiar Crystals of Sulphate of Potash.

By Richard Phillips, FRS. L. & E. &c.

THE crystals which I am about to describe were presented to me by Mr. Hills, of Bromley, patentee of the advantageous mode of preparing sulphuric acid from sulphuret of iron; they were formed by slow crystallization in a large quantity of the solution of the residuum, after preparing nitric acid from sulphuric acid and unrefined nitre.

From such a source, it would be expected that crystals of sulphate of potash only would be procured, but as the form of the crystals in question is totally different from that which sulphate of potash usually presents, it appeared to me possible either that they might contain water of crystallization, or excess of acid, that they might contain chlorine, or consist of a double salt with an alkali and earthy base; or, lastly, that the crystals might be a compound of sulphate of potash and sulphate of soda; some of these circumstances, it appeared to me, might arise from the impurities with which nitre in the state in which it is imported is known to abound.

The crystals are of various lengths; the longest is about eight inches, 7-8ths of an inch wide near the base, and about half an inch thick; they suffer no change by exposure to the air.

To determine whether they contain water of crystallization, of which sulphate of potash is well known to be devoid; 100 grains were subjected to a strong red heat in a platina crucible; the loss amounted scarcely to a grain: it is, therefore, evident that the crystals contain no combined water.

In order to discover whether the crystals contain any excess of acid, 100 grains were dissolved in water, the solution reddened litmus paper slightly; two grains of crystallized carbonate of soda were added to it; it then ceased to act upon litmus paper, and on the addition of two grains more of carbonate of soda, the solution reddened turmeric paper strongly. It is evident, therefore, that this small excess of sulphuric acid was in the state of mixture, and not of combination; indeed from the slight loss occasioned by exposure to heat, it was evident that the salt could not be bisulphate of potash, nor indeed does the form of its crystal more resemble that of the bisalt than of the common sulphate.

The salt does not appear to contain any combined chlorine; when nitrate of silver was dropped into a solution of it, it became slightly opalescent, but no precipitation occurred. To separate solutions of the salt, I added ammonia, carbonate of ammonia, and carbonate of soda, but no precipitation whatever occurred; therefore, the salt is not a double compound of an alkaline and earthy salt.

To determine whether the crystals are a compound of sulphate of potash and of sulphate of soda, it was necessary only to add the equivalent quantity of nitrate of barytes. Hydrogen being 1, an atom of sulphate of potash is 88, and that of sulphate of soda 72; consequently a salt constituted as I have supposed it possible this might be, would be represented by $88 + 72 = 160$; an atom of nitrate of barytes is 132; therefore 25 grains of the double salt ought to decompose 41·25 grs. of nitrate of barytes; I found, however, that considerably less of the barytic salt was sufficient; for when 40 grains of nitrate of barytes had been added to a solution of 25 of the salt, the filtered solution gave a copious precipitate on the addition of sulphuric acid.

From these experiments I considered the salt to be mere sulphate of potash. In order to arrive at a knowledge of the form of the crystals, I submitted them to the examination of my brother, Mr. William Phillips, who has favoured me with the following statement:

On the Form of the before-mentioned Crystals. By William Phillips, FLS. MGS. &c.

I return the crystals with drawings of their forms (figs. 1 to 6 inclusive, Pl. XVI), of about the size of the crystals themselves, but made somewhat roughly, not having been done according to rule, but only by the eye.

These crystals yield to mechanical division parallel to the planes M, T, and P, and the following admeasurements by means of the reflective goniometer of Dr. Wollaston, show that the primary form is a right rectangular, but not square prism, and especially the angles of *c* on *c′*, and *c′*, fig. 3, on the adjacent plane over the edge x.

The constant convexity of the plane *b* (which, however, from that circumstance, may rather be considered as a series of planes than as one plane) has rendered it impossible to obtain any measurement from its surface with another plane on any one of the crystals; while the nearly constant concavity of the plane *m* has rendered it almost equally impossible to ascertain its angle with any other plane accurately. Arguing, however, from analogy, we should assume that the plane *b* probably consists of a combination of the planes *m* and *n*, especially since the planes *a* and *c* appear at both extremes of the crystals, figs. 2 and 3. All the crystals of these forms show the same convexity on the planes on one side, and concavity on those of the other side, of the plane *d*.

The ordinary crystals of the sulphate of potash are in the form of two six-sided pyramids united at a common base, but sometimes separated by a short intervening prism, and variously modified, fig. 7. These crystals cleave parallel to the planes of the prism, and to that replacing the summit, that is, parallel to all the planes of a regular six-sided prism measuring on the lateral planes precisely 120° by the reflective goniometer. This circumstance, apparently so decidedly at variance with the cleavage of the crystals figs. 1 to 6, and the apparent incompatibility of their external forms with that of fig. 7, induced me to conclude that this substance must be peculiar in possessing two primary forms having no relation to each other; for it appeared certain, that, as the double six-sided pyramid may be cleaved parallel to all the sides of a regular hexadral prism, M M M, T, fig. 7, that that solid must be the primary form of these crystals, notwithstanding the almost constant appearance of a line passing along the plane *o*, in the direction of the dotted line on that plane of fig. 7, and which had tempted me to suspect the possibility of these crystals being macles, or hemitrope crystals.

After arriving at the fore-mentioned conclusion, I showed the sketches to my friend H. J. Brooke, Esq. who perceived a circumstance that had not made a sufficient impression on my mind; namely, that the measurement of the planes M on *d*, figs.

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SULPHATE OF POTASH.

Fig. 1.

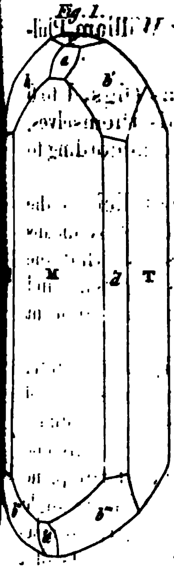


Fig. 2.

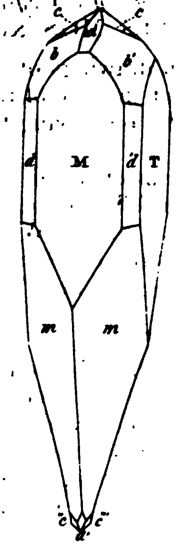


Fig. 3.

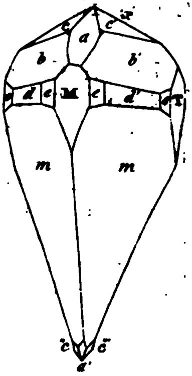


Fig. 4.

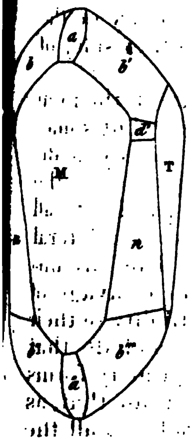


Fig. 5.

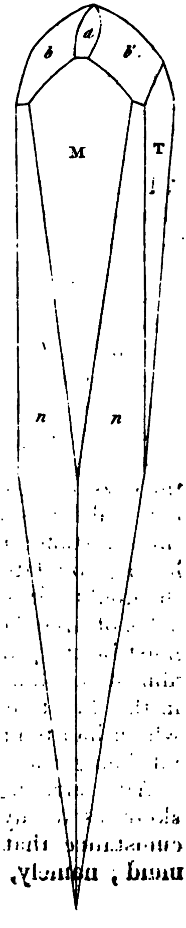


Fig. 6.

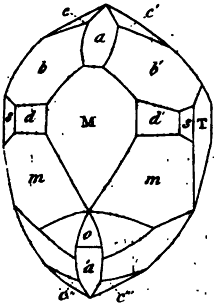
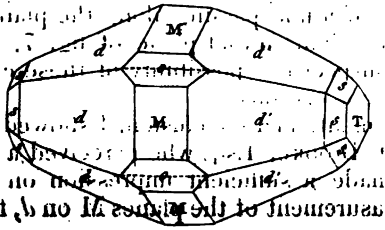


Fig. 7.



I to 5, corresponds with M or d, fig. 7, [illegible] fig. 7 is a [illegible] crystal [illegible] crystals so [illegible] M [illegible] M [illegible] [illegible]

[illegible]

5, corresponds with M on *d*, fig. 7, whence he inferred that 7 is a macled crystal consisting of the sections of several stals so united, that M on M or M is precisely 120°, and hence ecame apparent why these crystals should cleave parallel to se planes and to T, or, which is the same thing, to the planes six-sided prism. The measurement of M on *s*, fig. 7, since en, confirms the opinion that these crystals are in reality the les already described; and their structure will immediately perceived on comparing the planes M *d s* T with the same nes of figs. 3 and 6.

Figures 1 to 6.

M on T	90°	0′
P on M, or T	90	0
M on *d* or *d′*	146	30
M on *s*	127	0
M on *o*	150	0
M on *e*	150	0
T on *s*	143	10
T on *d*	123	30
M on *a*	137	20
M on *c*	114	0
M on *c′*		
c on *c′*	112	10
c on *c*	130	50
Over the edge x		
M on *m*	142	30
m on *m*	112	30
T on *m*	123	55
M on *n*	147	30
n on *n*	120	0

Figure 7.

M on T	90	0
M on *d* or *d′*	146	30
M on *s*	127	0
M on *o*	150	0
M on M	120	0

ARTICLE VI.

ract from a Memoir on the Composition of the Alkaline Sulphurets. By M. Berzelius.

(*Continued from* p. 288.)

THEREFORE, mixed some subcarbonate of potash with a t excess of sulphur, and heated the mixture until the sulphur fused; the combination then began to take place; the compound blackened, fused, and swelled. It was kept at this temperature until the fusion was complete; at this period, the lamp was withdrawn. After cooling, the mixture was found divided into two distinct layers; the upper was yellow, and consisted of sulphur; the lower was hepar, but which had not the bright red colour already mentioned. A portion of this hepar was dissolved in boiling water, precipitated by muriatic acid, boiled to expel the sulphuretted hydrogen gas, filtered, and evaporated to dryness. There remained upon the filter 0·734 gramme of sulphur, and the salt weighed 1·1 gramme. Having observed that when hepar is prepared in considerable quantity, the sulphate of potash is not found equally dispersed, I redissolved the salt obtained in water, and precipitated it by muriate of barytes: 0·321 gramme of sulphate of barytes was obtained, which is equivalent to 0·2415 gramme of sulphate of potash. There remains then, for the muriate of potash, 0·8585, containing 0·45 of potassium, which was combined with 0·734 gr. of sulphur: then

45 : 73·4 :: 100 : 163·11

But 164·24 constitute 8 atoms, and by adding the 2 atoms which escaped in the state of sulphuretted hydrogen, we have 10 atoms of sulphur for one atom of potassium. I observed afterwards that the compound which had a brighter-red colour, appears only when sulphur condenses during the cooling upon the hepar before it is quite hardened. As it does not form when the hepar fused under or with sulphur, and as the water does not combine with its excess of sulphur, it appears to be a mixture of hepar and sulphur, the brighter colour of which is derived from the latter substance, but which does not form a real and determinate compound.

I have already observed that when the sulphate of potash is decomposed at a high temperature by sulphuretted hydrogen gas, a bright hepar is obtained, which is perfectly transparent, and of an orange colour, and appears to be KS^7; and that when the same salt is decomposed by sulphuret of carbon, $K\,S^8$ is formed: this is not transparent, and its colour is not so fine as the former. In these operations, the same proportion of sulphur in excess usually occurs. Carbonate of potash weighing 0·7815 gramme was fused with 1·5 gramme of sulphur, in sulphuretted hydrogen gas. The excess of sulphur was expelled, and sulphuretted hydrogen gas was passed over the fused mixture as long as water was formed. This fluid was always accompanied by sulphur, which was deposited as long as water was produced. The operation being finished, the mixture weighed 1·18 gramme. It contained 0·442 gr. of potassium, which was consequently combined with 0·738 gr. of sulphur: but

44·2 : 73·8 :: 100 : 166·9

and 164·24 constitute 8 atoms. As in this operation, $\ddot{K}\,\dddot{S}^2 + 3\,K\,S^{10}$ is at first formed, and as the atom of sulphate of potash is afterwards reduced to $K\,S^2$, the whole ought to form $K\,S^8$; but what shows that it is a decided compound, and not a mere mixture, is that all the sulphur of the sulphuretted hydrogen escapes with the water; and consequently no $K\,S^7$ is formed, as happens when the sulphate of potash only is decomposed. The compound becomes opaque after cooling.

The hepar obtained in the preceding experiment was afterwards mixed with half its weight of sulphur, the mixture was distilled until sulphur ceased to come over, when sulphuretted hydrogen gas was passed through the apparatus. The hepar then weighed 1·259 gramme; that is, 100 parts of potassium were combined with 184·57 of sulphur, which makes exactly 9 atoms.

Two grammes of bicarbonate of potash decomposed by sulphuretted hydrogen in a similar apparatus gave 1·49 gramme of a pale-yellow crystalline salt, in which 100 parts of potassium were combined with 91 of sulphur and of hydrogen, as we shall presently see. I added one gramme of sulphur, and the mass was again fused in a current of sulphuretted hydrogen gas, until the distillation of sulphur ceased. The matter then weighed 2·243 grs.; thus 100 parts of potassium were combined with 186 parts of sulphur; which also makes 9 atoms.

In a weighed retort 1·079 gramme of carbonate of potash was fused with 0·302 gr. of sulphur. Combination immediately took place at a temperature scarcely exceeding that requisite for the fusion of sulphur, and the mixture was exposed during an hour to the same degree of heat; for when it was stronger, the carbonic acid gas was extricated rapidly, carrying with it much sulphur in the state of a white vapour. The temperature was raised afterwards until the mass fused: when it had been thoroughly liquefied, and bubbles ceased to appear, the operation was stopped. The apparatus had lost 0·165 gramme in weight, which was carbonic acid that had escaped. This corresponds to 0·3535 of potash, of which one-fourth equal to 0·08838 combined with the oxygen of the remainder, and to 0·02933 of sulphur, formed sulphate of potash. There remain then 0·302 − 0·0293 = 0·2727 gramme of sulphur which were combined with 0·22 gr. of potassium; then

$$22 : 27{\cdot}27 :: 100 : 123{\cdot}99$$

and 123·18 indicate 6 atoms of sulphur.

The same experiment was once more repeated, and gave the same result. I employed more potash than the sulphur was capable of decomposing: thus the affinity of the carbonic acid for the potash prevented the combination of the sulphur with the potash and its base. Then, when sulphur is fused at a low red heat, with more carbonate of potash than it is capable of decomposing, it forms $K\ S^6$. It follows also from this experiment, that 100 parts of subcarbonate of potash are decomposed at a low red heat, by 58·22 of sulphur, and it forms then $K\ S$ + $3\ K\ S^6$.

When this compound is mixed with an excess of carbonate of potash, and heated to redness, it boils slowly, and gives out some carbonic acid gas; but as at this temperature the glass is acted upon, it is difficult to determine whether the evolution of carbonic acid gas arises from this cause, or from the formation of a lower sulphuret of potassium. The experiment was repeated in a small platina crucible, in which 3·7 grammes of carbonate of potash were mixed with 0·5 gramme of sulphur. The crucible was placed in another, and surrounded with powdered charcoal, and these were put into a third crucible; they were all perfectly closed with lids. The object of this arrangement was to prevent the access of atmospheric air during cooling. The mixture was heated for a long time at a very low temperature to prevent the loss of sulphur which might occur during the extrication of the carbonic acid gas; the heat was afterwards gradually increased to redness, and kept up to this degree for half an hour. The weight of the cooled crucible was diminished by 0·364 gramme, caused by the disengagement of carbonic acid gas, and corresponding to 0·78 gramme of carbonate of potash. Of this quantity, one-fourth, or 0·195 gramme, had combined with 0·066 gr. of sulphur to form sulphate of potash, so that 0·434 of sulphur were combined with 0·485 gr. of potassium; but

$$48{\cdot}5 : 43{\cdot}4 :: 100 : 80{\cdot}95$$

Then 82 would indicate 4 atoms, and the difference may arise from the disengagement of sulphur which might have accompanied the carbonic acid gas at the commencement of the operation, when its escape could not be discovered, nor the heat regulated.

The result of this experiment is then, that when $K\ S^6$ mixed with carbonate of potash is exposed to a red heat, an additional portion of carbonate is decomposed, and $K\ S^4$ is formed, a degree of combination in which the affinities of the sulphur and carbonic acid are counterbalanced; 100 parts of carbonate of potash form then with 43·78 parts of sulphur

$$\dot{K}\,\dddot{S}^2 + 3\ K\ S^4$$

The hepar obtained by this operation is green, a colour which evidently does not belong to the sulphuret of potassium. When it had been dissolved in water, it deposited a spongy substance, of a reddish-brown colour, which was hydrosulphuret of platina. The crucible had lost 0·3 of a gramme in weight. There was, therefore, formed a double sulphuret of potassium and platina

$K S^4 + Pt^2$; but that which clearly proves that this compound was formed after that of $K S^4$, is that in the contrary case, the two atoms in excess of $K S^6$, could not to expel the carbonic acid from the potash; they would have no action upon it, exactly as if $K S^6$ only was formed, and besides one-third of $K S^4$ could not combine with the platina, at the end of the operation, without being accompanied by some change in the weight. It is seen by this how difficult it is to obtain $K S^4$, at least by this method, because in glass vessels the silica of the glass opposes its formation, and in metallic vessels, the sulphur is divided between the metal and the potassium, forming a double sulphuret.

These experiments also show the action exerted by the alkaline sulphurets in fusion upon the metals: the metal, provided it be in sufficient quantity, divides the sulphur with the potassium, until $K S^2$ is formed. In these double sulphurets, the number of the atoms of the metallic sulphuret produced is determined by the number of atoms of sulphur in the sulphuret of potassium. Experience has shown that many of these double sulphurets are decomposed by water, which separate some sulphurets such as those of lead, silver, and copper; while others are dissolved, as the sulphurets of arsenic, tungsten, tin, and gold. We shall hereafter refer to these interesting compounds.

We have then acquired a knowledge of several sulphurets of potassium, which contain 2, 4, 6, 7, 8, 9, and 10 atoms of sulphur.

1. $K S^2$, obtained by reducing sulphate of potassium by hydrogen.

2. $K S^4$, by fusing carbonate of potash in a red heat, with a quantity of sulphur less than required to decompose it.

3. $K S^6$, by heating the above mixture slowly, until it fuses, and without ebullition or evolution of any gas.

4. $K S^7$, by reducing sulphate of potash with sulphuretted hydrogen gas.

5. $K S^8$, by keeping the hepar at a maximum in fusion in sulphuretted hydrogen gas, until neither water nor gas are disengaged, or by reducing sulphate of potash with sulphuret of carbon.

6. $K S^9$, by fusing the preceding with sulphur, the excess of which is driven off by a moderate heat, a current of sulphuretted hydrogen gas, or any other gas which contains no oxygen, being passed over the liquefied mass.

7. $K S^{10}$, by the fusion of carbonate of potash with an excess of sulphur, till no carbonic acid gas is evolved. It is not requisite to raise the temperature above a red heat, to decompose the salt completely.

The combinations in which the sum of the atoms of sulphur is expressed by even numbers, answer to 1, 2, 3, 4, and 5 atoms of sulphur for each atom of potassium, potash being considered as composed of one atom of metal and an atom of oxygen. These combinations agree with both modes of reckoning the atoms, and the methods of obtaining them are such as necessarily produce compounds of determinate proportions.

As to those in which 1 atom of potassium is united to 7 or 9 atoms of sulphur, they prove incontestably the justice of the opinion, that potash contains not 1 atom but 2 atoms of oxygen; for in the first case, these compounds would contain $3\frac{1}{2}$ and $4\frac{1}{2}$ atoms of sulphur, and we cannot admit of half atoms. I am, however, far from regarding the thing as proved by these compounds, especially since we know that magnetic oxide of iron, both natural and artificial, is composed of two different oxides of iron, and it is consequently possible that the above combinations may contain two degrees of sulphuration, which might be either entirely similar to the two compounds $K\,S^7$ and $K\,S^9$, or at least very nearly resembling them.

I ought not, however, omit to observe, on this occasion, that in all my experiments, with very few exceptions, the quantity of sulphur has very little exceeded that indicated by calculation. This happens either because the atom of sulphur is actually heavier than it has been reckoned, or which seems more probable, because the last portion of sulphur is expelled with difficulty.

III. *Combination of Sulphuretted Hydrogen with Potash.*

I have already shown that the subcarbonate of potash decomposed by sulphuretted hydrogen gas, gives a hepar of a bright-yellow colour, which crystallizes on cooling, and which has a crystalline fracture like that of salts. 20·87 grammes of subcarbonate, subjected to a dull red heat, were exposed to a current of sulphuretted hydrogen gas until no water was produced; it was not accompanied by sulphur. The excess of sulphuretted hydrogen gas escaped without having undergone any alteration, and merely mixed with carbonic acid gas: the action of the heat was continued for about six hours. The fused mass was in a state of continual ebullition, which was undoubtedly caused by the evolution of carbonic acid gas and aqueous vapour; for when at length it ceased, no further disengagement of water occurred. Gas was constantly passed through the apparatus until it became cool. The compound was of a pale lemon-yellow colour; it was crystallized in large brilliant plates; it weighed 22·28 grammes, was very deliquescent, and dissolved in water, to which it communicated a pale-yellow colour.

20·87 grammes of carbonate of potash contain 11·816 grs. of potassium, which quantity remains of course in the 22·28 of sulphuret of potassium obtained. It was, therefore, united to 10·464 gr. of sulphur; but

$$11·816 : 10·464 :: 100 : 88·5$$

Four atoms of sulphur would be 82·12; there is in this case the difference of 6·43. I took this combination at first for $K S^4$, but having mixed a portion of the solution with nitrate of copper, it formed to my great surprise a precipitate of sulphuret of copper. Other metallic salts produced a similar effect; consequently the solution contained more sulphuretted hydrogen than had been formed by the oxidation of the potassium. Mixed with an acid, it became turbid and milky; but the precipitated sulphur formed only a very small deposit; and the remainder of this substance escaped in the form of sulphuretted hydrogen gas.

It was then evident that the combination effected in the dry way was composed of sulphuret of potassium and sulphuretted hydrogen; or if we suppose it to be a double sulphuret $K S^2 + 2 H^2 S$; that is to say, that the potash and hydrogen combine with an equal quantity of sulphur, it follows that 100 parts of potassium combine with 82·12 of sulphur, and with 2·60 of hydrogen, making together 84·72 parts. The excess was undoubtedly derived from access air, which, by oxidizing the hydrogen at its expense, occasioned a higher degree of sulphuration, the precipitate produced by acids.

It became then interesting to discover if the neutral hydrosulphuret of potassium is similarly constituted. With this view, I saturated a portion of pure potash with sulphuretted hydrogen gas, and heated the mixture to ebullition, passing a current of hydrogen gas through the apparatus until all the excess of sulphuretted hydrogen gas was expelled. One part of this solution was precipitated by muriate of copper added drop by drop. The precipitate was collected on a filter, well washed, dried, and heated in a retort, until nothing but sulphuret of copper at a *minimum* remained; it weighed 1·82 gramme. After the remainder of the copper had been separated by sulphuretted hydrogen from the solution, it was evaporated to dryness, and gave 1·71 gramme of muriate of potash: there were, therefore, 2 atoms of copper to 1 atom of potash. It is evident from this, that to form a neutral hydrosulphuret, the potash takes such a quantity of sulphuretted hydrogen, as that the hydrogen is double the quantity which is required to form water with the oxygen of the potash, and that this hydrosulphuret in its dry state may be represented, like the preceding combination, by $K S^3 + 2 H^3 S$.

It is well known that the composition of sulphuret of potassium at a minimum is such, that when it is destroyed by water, an hydrosulphuret is formed in which the potash is half saturated with sulphuretted hydrogen when compared with the above compound. We here meet with the two degrees of saturation which M. Gay-Lussac has mentioned, but the composition of which he has not described. We shall in the sequel examine, whether they are actually what they appear to be; that is to say, whether they are hydrosulphurets.

IV. *Formation of Hepar in the Moist Way.*

Hepar may be obtained by two modes in the moist way, either by boiling hydrosulphuret of potash with sulphur, or by boiling or fusing at a moderate heat hydrate of potash with sulphur. We shall examine these processes.

1. The solution of sulphuret of potassium at a minimum is represented by $\ddot{K} + 2\, H^2\, S$, which I shall call subhydrosulphuret of potash. When this solution in a concentrated state is digested with a little sulphur, the latter is dissolved, and we obtain by this process sulphuret of potassium of every degree, so that the solution may contain 4 atoms of hydrogen and 10 atoms of sulphur for each atom of potash, or $\ddot{K} + H^4\, S^{10}$, which is the same compound as that which is formed when the sulphuret of potassium at a maximum is dissolved in water.

2. When the neutral hydrosulphuret of potash $\ddot{K} + 4\, H^2\, S$, in the state of concentrated solution is mixed with powdered sulphur, there results strong effervescence, even at common temperatures; sulphuretted hydrogen gas is evolved, the sulphur is dissolved, and the solution becomes of an orange colour. If sulphur be added as long as the evolution of the gas continues, we obtain the compound $\ddot{K} + H^4\, S^{10}$; so that 8 atoms of sulphur expel 2 atoms of sulphuretted hydrogen, or one half of the hydrosulphuric acid contained in the salt.

3. Sulphur digested with hydrate of potash is dissolved. A part of the sulphur is acidified in the first degree, and forms hyposulphurous acid. If it be supposed that this oxidation occurs at the expense of the water, its hydrogen then serves to form hydrosulphuric acid, which saturates a part of the potash, and this compound then dissolves, as we have already seen, an additional quantity of sulphur. When it is saturated, we have the compound $\ddot{K} + H^4\, S^{10}$. If the quantity of sulphur be smaller, inferior sulphurets are formed.

It is natural to suppose that sulphurous acid might also be formed in this operation; I, therefore, attempted but without success, to obtain sulphurous or sulphuric acid, by boiling or fusing hydrate of potash with very small quantities of sulphur. The strongly alkaline solution obtained was mixed with hydrate of copper till it became colourless; it was filtered and supersaturated with muriatic acid. Much sulphur was precipitated, and sulphurous acid was at the same time evolved. Such being the case, although only a small quantity of sulphur dissolved, there is no reason to suppose that sulphurous acid is formed in any of the above descriptions.

In order to determine the proportions in which the hyposulphurous acid and sulphuretted hydrogen are combined with potash in the highest degree of saturation, I dissolved some sulphur in a solution of potash contained in a vessel provided with a valve which suffered vapours to escape, but suffered nothing to enter. When the sulphur ceased to dissolve in the boiling liquid, it was suffered to cool. A portion of it was decomposed by hydrate of copper; the liquid, when filtered, was treated with *aqua regia*, and poured into a well-stopped flask. It became turbid and milky; after some hours, it was boiled in the unstopped flask; a small portion of sulphur weighing 0·046 gramme was deposited. The solution was mixed with muriate of barytes, which precipitate 0·95 gramme of sulphate of barytes. The filtered solution was precipitated with excess of sulphuric acid; then again filtered and evaporated to dryness; after this, it was dried with the requisite care, so that there remained merely neutral sulphate of potash; it weighed 1·287 gramme. I repeated this experiment with nearly similar results; that is to say, the sulphate of barytes weighed one per cent. more than the sulphate of potash. This circumstance can only be explained by supposing that the saturated hepar contained $\ddot{K}\ S^6 + 3\ \ddot{K}\ H^4\ S^{10}$, and that in this case, as well as in the dry way, one-fourth of the potash combines with hyposulphurous acid, in such proportion that the acid contains three times as much oxygen as the base. It follows that we ought to obtain by analysis three atoms of sulphate of barytes for four atoms of sulphate of potash, the weights of which are to each other as 874·8 : 872·8. Consequently hyposulphurous acid may combine with bases in three proportions: first, that which occurs when zinc or iron is dissolved in sulphurous acid, in which the base and the acid contain an equal quantity of oxygen; secondly, that which is formed when sulphur dissolves in sulphurous salts, or when hepar oxidizes in the air; in this the acid contains twice as much oxygen as the base; thirdly and lastly, the case which has just been mentioned, in which the acid contains three times as much oxygen as the base. It is clear that if hydrate of potash be added to saturated hepar, a hyposulphate less saturated with acid is formed, while the hepar itself suffers no alteration, because the relation of the hydrogen undergoes no change. This circumstance may occasion the question, if with less sulphur there would be formed, for example, $\ddot{K}\ \ddot{S}^2 + \ddot{K}\ H^4\ S^2$, or $\ddot{K}\ \dot{S}^4 + \ddot{K}\ H^8\ S^4$; this, however, does not appear to happen, for the smallest quantity of sulphur colours the potash, and these combinations would be colourless; or there might be formed $\ddot{K}\ \ddot{S}^4 + 2\ \ddot{K}\ H^4\ S^4$ or $\ddot{K}\ \dot{S}^4 + \ddot{K}\ H^4\ S^6$, and so on with an increasing number of atoms of sulphur up to 10. Indeed it is necessary only to add a portion of potash corresponding to the weight of $\frac{1}{2}\ K$ or half an atom of potassium to the saturated hepar already mentioned, to obtain the stated relation between the quantity of base which combines with the acid, and that which combines with the hydrogen in its different degrees of sulphuration. It may then be considered as certain, that all these latter compounds are formed on account of the different quantities of sulphur present. But another question arises. May not potassium combine with more than 10 atoms of sulphur? We have seen that by the dry way it cannot. When a drop of sulphuric acid is put into a solution of hepar prepared in the dry way, it becomes immediately turbid, and the precipitate is insoluble; therefore water cannot hold in solution a sulphuret of a higher degree; if, on the contrary, we boil a moderately concentrated solution of hydrate of potash with sulphur to perfect saturation, a certain portion of the sulphur is precipitated during cooling; but its quantity varies according to the concentration of the liquor. If the hot solution be poured into a cold vessel, sulphur is immediately deposited throughout the liquid; but this sulphur is partly precipitated by the influence of the air, which acts principally while the mass is hot. A solution of potash in alcohol dissolves much more sulphur than an aqueous solution of the alkali. It deposits much sulphur during cooling, and even when the solution is diluted with water. The hepar is rendered turbid in general when it is mixed with a large quantity of water by the action of the air which the water contains. If the hepar at a *maximum*, prepared in the dry way, be dissolved in alcohol, and it be left in a bottle which is not well stopped, small radiating colourless crystals are formed after some hours on the surface of the liquid; but no sulphur is deposited. These crystals are hyposulphate of potash, and the sulphur which would otherwise precipitate is held in solution by the alcohol, until it is saturated; at this period the sulphur and the hyposulphate begin to crystallize together; and this continues until the liquor becomes colourless. From these experiments nothing can be concluded with certainty as to the existence of a sulphuret of potassium exceeding $K\,S^{10}$.

Former observations have shown that lime cannot in the dry way be combined with a large quantity of sulphur: this has been confirmed by the experiments of M. Vauquelin, related in his memoir already quoted. I have already shown that when lime is reduced by sulphuretted hydrogen, $Ca\,S^2$ is formed; but I was unable to cause this compound to take a larger quantity of sulphur. When hydrate of lime is boiled even with excess of sulphur, there are generally two compounds formed, one of which is but slightly soluble, and is partly deposited during ebullition in the state of a deep coloured yellow powder, and partly during cooling in crystals of the same colour. This salt was first described by Buchner. Doebereiner offered some conjectures as to its composition and its form, was determined by Bernhardi; at last Herschel discovered by his experiments that it was composed of $\ddot{C}a\,H^4\,S^4$. The part which remains in solution is a sulphuret of a higher degree, but the composition is difficult to determine, because part of the hyposulphite remains in solution. To avoid this inconvenience, I boiled sulphuret of calcium ($C\,a\,S^2$) (prepared with pure lime heated in a current of sulphuretted hydrogen gas) with sulphur in excess, until it was saturated; I decomposed the solution by muriatic acid, separated and weighed the sulphur, and converted the muriate of lime into sulphate. I obtained 1·682 gramme of sulphur, and 1·815 gramme of gypsum. These make 8 atoms; for an atom of gypsum (equal to 1714·38) is to 8 atoms of sulphur, represented by 1601·9 as 1·815 is to 1·690. Adding 2 atoms of sulphur, which disappeared in the form of hydrogen gas, we find 10 atoms, and the hepar of lime at a maximum is also composed of $\ddot{C}a\,H^4\,S^{10}$.

In general only two determinate compounds can be prepared in the moist way, one with 10 atoms, and the other with 4. The latter is obtained with potassium and sodium, by exposing the neutral sulphuret to the air, until half its hydrogen is oxidated and converted into water, by means of which $\ddot{K}\,H^4\,S^4$ is formed. This compound may be obtained with lime and strontian, as MM. Herschel and Gay-Lussac have proved, by boiling the earth with sulphur, and suffering the solution to cool; this compound then crystallizes. The intermediate degrees are obtainable only by mixture in proportions determined by calculation.

The nature of these solutions may be considered in two modes, and it is at present impossible to decide which is the correct one: first, either water is decomposed by the sulphur when the combustible body is dissolved by the alkali, or by the base of the alkali when the metallic sulphuret is treated with water; secondly, or the metallic sulphuret dissolves in water without being altered, and the sulphuretted hydrogen, which the acids evolve from the solutions, are formed only at the instant in which the potassium is oxidated by means of the acid.

In the first hypothesis, the hepar is a compound of potash and sulphuretted hydrogen; but then this latter body cannot be considered as the only one of its kind. There must be as many degrees of sulphuration for hydrogen as there are for potassium; that is to say, if we except the odd numbers 7 and 9 from the preceding experiments, there must exist compounds of two atoms of hydrogen with one, two, three, four, and five atoms of sulphur, each forming peculiar salts. It is evident that the denominations of *hydrosulphates* and *hypohydrosulphates* are no longer proper. It would be more correct to call these different compounds *hydrosulphurets*, *hydrobisulphurets*, *hydrotri*, &c.

I made several vain attempts to obtain the compounds of hydrogen in an isolated state; they separated always into sulphuretted hydrogen gas and an oily compound, in the same manner as the peroxide of hydrogen. This compound cannot exist unless with an acid, and even then it exists but a few hours, except under strong pressure. In the experiments which I performed to discover the nature of this body, I found that the best method of obtaining it is to pour the saturated hepar ($K S^{10}$) by small quantities at a time, into a warm mixture of muriatic acid and water. The acid should be neither too much concentrated nor too dilute. The heat is so far from accelerating the decomposition, that it causes the separated body to remain in drops; and although a little sulphuretted hydrogen is disengaged, and sulphur deposited in the solution, the greater part nevertheless consists of this oil, which has a yellowish colour, and which, when the experiment succeeds well, is nearly transparent. This oil, afterwards heated in an acid, suffers a little sulphuretted hydrogen to escape; but a small quantity only is decomposed, before the water begins to boil, and then the aqueous vapour conducts a little sulphuretted hydrogen. When collected on a filter, it has the appearance of an oily substance; it is not very fluid, and does not become solid till some days have elapsed; it has a peculiar disagreeable smell, totally different from that of sulphuretted hydrogen; when it is heated, it affects the nose and eyes nearly in the same way that cyanogen does. Similar effects are produced by the vapours of the acid liquor when the oily substance is boiled in it; if it be received upon a cold body, the drops become milky, and the effects are particularly evident after the free sulphuretted hydrogen is evaporated from the liquor.

The composition of this body cannot be determined with certainty. The circumstances of its preparation show that it contains at the period of its formation at least five atoms of sulphur for two atoms of hydrogen; and that it afterwards undergoes a change in the proportions of its constituent parts, by the loss of sulphuretted hydrogen. It resembles the peroxide of hydrogen in this circumstance, that by admixture with water, it is gradually resolved into sulphuretted hydrogen and sulphur; treated in the cold with an alkali, it almost immediately becomes fixed; the alkali unites with the sulphuretted hydrogen, and leaves the sulphur. It is remarkable that the compound of sulphur and hydrogen of the hepar at a *maximum*, or which is formed there, consists of $2 H + 5 S$, and that it is consequently similar in composition to nitric acid $2 Az + 5 O$, and probably to the arsenic and phosphoric acids.

If, on the other hand, we suppose that the alkaline sulphurets are dissolved in water without their being decomposed, it follows that no similar hydrosulphurets exist; the compounds of hydrogen and sulphur in so many proportions are scarcely necessary; but an acid poured upon the hepar produces upon the sulphuret of potassium the same effect as upon the sulphuret of iron, and the sulphuretted hydrogen is formed only at that moment. The effect of the acid upon the dry hepar is perfectly similar to that produced upon the dissolved hepar; but, therefore, remains for us to examine, if a similar opinion has any probability.

I have shown in a former memoir, that it is at least extremely probable, that the double cyanuret of iron and potassium, the sulphocyanuret of potassium, &c. dissolve in water without decomposition, and are deposited in the state of crystals without the potassium being oxidated, and without the cyanogen or sulphuretted cyanogen combining with hydrogen to form acids. If the compound of potassium with a combustible body acts thus, it is not impossible that another compound might be similarly circumstanced; but it may be possible without actually occurring.

Boiling water, when poured upon sulphuret of potassium, dissolves but a very small portion of it; the insoluble suffers no change either of colour or composition. I preserved sulphuret of calcium for several months in a stopped bottle, full of water, without this substance being decomposed. If then it was really decomposed by water, it would appear that this decomposition ought to happen, even when the hydrosulphuret of lime formed is but slightly soluble in water, especially also as barium, calcium, manganese, &c. decompose water, and evolve hydrogen, although the oxide formed at the same time is not soluble. The solution of sulphuret of calcium obtained is colourless. When evaporated in vacuo over sulphuric acid, it is deposited in small white scaly crystals upon the sides of the vessel; these crystals when slightly heated part with the water, pass again to the state of sulphuret of calcium, in the same way as a salt with water of crystallization, or like the double cyanurets of iron with potassium, barytes, or lime. It is then at least as probable that the sulphuret of calcium dissolves in water without undergoing any change, and may combine with water of crystallization, as it is that this sulphuret should be decomposed by water into an hydrosulphuret.

As to the sulphuret of potassium, it seems to act differently; for this compound is deliquescent, and, therefore, nothing can be concluded from it. In order to come near the truth, I fused the hydrate of potash in a small retort with a spirit lamp, and I added the sulphur in small portions; (on the introduction of each piece of sulphur) the matter boiled owing to heat excited by the combination; aqueous vapour was formed; the salt assumed a yellowish colour, and separated a white caseous substance, which swam on the surface of the liquid, and by ebullition it was carried still higher on the side of the glass. The operation was discontinued, while there still remained hydrate of potash in great excess. The white matter which was separated was easily dissolved in water, and the solution was colourless; it was precipitated by muriate of barytes, but the precipitate was dissolved by muriatic acid, and in a short time the solution was rendered milky with sulphur, and sulphurous acid was given out.

The compound of potash, when cooled, was of a pale cinnabar-red colour, and when dissolved in water, the solution was colourless. In this case there was formed, *not* the hydrosulphuret of potash which is colourless, but sulphuret of potassium at a *minimum* $K S^2$ which is red, and consequently may be fused with hydrate of potash, as we have also seen that it may be with sulphate of potash. But if, at this high temperature (when the water is ready to evaporate), it is not the liquid, but the potash which is decomposed, and a hyposulphite, and a sulphuretted metal is formed, why should not this occur at a low temperature when the water further from decomposition has a greater affinity for the substances dissolved? But if the sulphuret of potassium may be mixed with other oxidated bodies, it may dissolve or be dissolved by them; as, for example, the hydrate, sulphate, and carbonate of potash, as we have already seen in decisive examples, why should we not admit that it may mix with water, and dissolve in it? Admitting this fact, it will follow from it, that the series of compounds of sulphur and hydrogen which have been mentioned may exist; but it will not necessarily follow that hydrogen can combine with sulphur in as many proportions as potassium. In every case the formation of sulphuretted hydrogen is derived from the action of acids; in the same way, for example, as the sulphuretted hydrocyanic acid, although a well characterized acid, is instantly destroyed when mixed with potash, and occasions the formation of sulphocyanuret of potassium, but is again formed when an acid is added. On the other hand, we have the corresponding compounds of ammonia, with their different quantities of sulphur, and with hydrogen; when, after separating ammonia, there remain several sulphurets of hydrogen. But if the ammonia is not an oxide, and if the metal which deposits upon the mercury at the negative pole of the electric pile is composed of $Az + 4H$, the different degrees of sulphuration of ammonia must also be considered as solutions of a metallic sulphuret, but of a sulphuret with a compound base. In a word, the more this subject is examined, the more difficult it is to give a decided preference to either of the two explanations; and, for the present, it will, perhaps, be better to confine ourselves to the study of them.

It is extremely probable that the greater number of bodies may combine together in an equal number of proportions, that the metals have an equal number of oxides and of sulphurets, and that we are acquainted with but few of them, because we have not discovered the means of producing those compounds which are most readily decomposed, on account either of the weakness of the affinities, or of the mechanical construction of the compound atom. The study of the properties of hepar strengthen this idea. Several metals have hitherto given us but one sulphuret, as, for example, lead and silver; but with the hepar we may precipitate these metals from their solutions, combined with the same number of atoms of sulphur as the potash contained. It is thus, for example, that lead precipitates with 10 atoms of sulphur, of a fine blood-red colour; but this compound exists only for a few seconds, and is soon converted into a mixture of common sulphuret and sulphur. The persulphurets of other metals are more permanent; that of copper, for example, which is liver-coloured, and unalterable both by the air, and by boiling water. It would be extremely interesting to ascertain the higher sulphurets produced in this manner, and the difference which exists between the sulphurets of various degrees. I precipitated salts of copper, by $K\,S^4$, $K\,S^6$, and $K\,S^{10}$; but all the precipitates resembled each perfectly in colour, and dissolved in carbonate of potash forming a brown solution.

It may be concluded with certainty from the experiments which I have detailed, that sulphur cannot combine with an oxidated body, and that consequently there exist no alkaline sulphurets; but when a salifiable base takes sulphur in the dry way, it is partly reduced, and a sulphate and a metallic sulphuret is formed. In the humid way either the same reduction occurs, or water is decomposed, and a part of the base unites to a compound of sulphur and hydrogen; while the other part combines with the hyposulphurous acid, which is produced at the same time.

(To be continued.)

ARTICLE VII.

Astronomical Observations, 1822.

By Col. Beaufoy, FRS,

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Oct. 3.	Eclipse of Jupiter's third satellite	Immer.	9h	27′	51·4″	Mean Time at Bushey.
		Immer.	9	29	12·3	Mean Time at Greenwich.
		Emer.	11	35	11·9	Mean Time at Bushey.
		Emer.	11	36	32·8	Mean Time at Greenwich.
Oct. 8.	Eclipse of Jupiter's first satellite	Immer.	10	22	37·7	Mean Time at Bushey.
		Immer.	10	23	58·7	Mean Time at Greenwich.
Oct. 8.	Eclipse of Jupiter's second satellite.	Immer.	12	55	40·6	Mean Time at Bushey.
		Immet.	12	57	01·6	Mean Time at Greenwich.
Oct. 10.	Eclipse of Jupiter's third satellite	Immer.	13	27	46·2	Mean Time at Bushey.
		Immer.	13	29	07·1	Mean Time at Greenwich.
		Emer.	15	34	01·1	Mean Time at Bushey.
		Emer.	15	35	22·0	Mean Time at Greenwich.

ARTICLE VIII.

An Account of several Circumstances connected with the Ductility of Glass. By John Deuchar, MRAI. Ed. Cal. Hort. Soc. and Wernerian Nat. Hist. Soc.; and Lecturer on Chemistry in Edinburgh.*

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Edinburgh, Oct. 18, 1822.*

THE great ductility of glass seems, at an early period of the history of that compound, to have been noticed by philosophers; but they have entirely overlooked several very important accompanying circumstances. They had, in the construction of the thermometer and other instruments, found that a hollow ball could be drawn out till it formed a very long tube still hollow; but they made no attempts to ascertain the extent to which the ductility of a tube might be carried without the hollow part being closed up, nor if it were at all changed in its relative dimensions. Thus their knowledge of the ductility of hollow glass appears to have been confined to the observation, that a melted tube drawn out by the fingers till it formed very brittle threads, still admitted of the air being blown through it.

But, with regard to solid glass, no experiments whatever seem at that early period to have been tried to ascertain its ductility. This attempt was left for modern ingenuity; at first, about 40 or 50 years ago, it was performed by means of the fingers; and the late Mr. Knee, of Edinburgh, was the only person who did so to any great extent. A mode, however, was introduced about 20 years afterwards, by means of which the glass was drawn out upon a wheel with greater rapidity than common threads. This method of spinning glass, as it is now termed, was exhibited in Scotland by Mr. Gheri in 1808, by Mr. Finn in 1811, and by Mr. Davidson in 1812; and I have availed myself of their assistance in drawing such of the specimens of spun glass, noticed in the present experiments, as are not of my own manufacture.

I was led to examine the subject from the different appearance I observed in the threads drawn from a piece of window glass with sharp angles, and those drawn from a circular piece of crystal equally transparent; the former having great lustre, and the latter presenting a dull surface.

Although 13 years have now elapsed since I commenced the investigation, during eight of which I have been in the practice of showing the experiments in my classes, yet no treatise upon glass, so far as my information goes, has thrown out any hint on the subject; on which account I have been induced to lay the present facts before the Society; it would be tedious and unnecessary to occupy the time of the Society in giving an account of the whole of the numerous experiments in which I have been engaged. I shall, therefore, select a few of the more striking.

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* Read before the Wernerian Natural History Society, May 18, 1822.

Exper. 1.—Some of the hollow glass threads were put into distilled water, and then placed under the receiver of an air-pump; upon the air being withdrawn from the receiver, bubbles of air issued from the ends of the glass threads, and continued to do so as long as the exhaustion was kept up.*

Exper. 2.—In another experiment, 20 grains of glass thread, drawn from a tube, fig. 6, were kept at the bottom of a jar of mercury, under the receiver of an air-pump, and the air was withdrawn; the glass was weighed after the experiment, and found to have increased to more than twice its former weight from the mercury which then occupied the space from which the air had escaped. The hollow threads are more brittle than the solid.

Fig. 1.

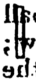


Fig. 2.

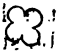


Fig. 3.

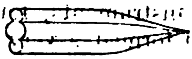


Fig. 4.

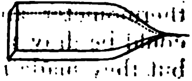


Fig. 5.

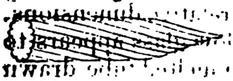


Fig. 6.

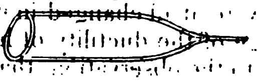


Fig. 7.

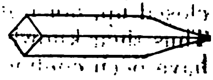


Fig. 8.

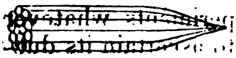
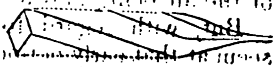


Fig. 9.



Exper. 3.—Another experiment may be selected which shows the result in a more striking point of view; a piece of a thermometer tube, the bore of which was very small, was drawn into threads remarkably fine. The wheel, round which the threads were spun, had a circumference of three feet, and this, making 500 revolutions in a minute, proved that 30,000 yards of the glass had been drawn round it in an hour; and as the state of fusion and quantity fused at a time of the glass is the same, whether the drawing be rapid or slow, it follows that in this example the thread must have been very fine, and its bore almost incalculably minute. Some of this thread was cut into pieces an inch and a half long, and these so situated on the top of the receiver of an air-pump, that the one end of each tube communicated with the interior, and the other with the exterior of the receiver; to make the result still more satisfactory, a few of the threads had their under ends bent out from the rest; mercury was then poured over the upper ends of the glass threads, and the air thereafter exhausted from the receiver; upon which being done, the mercury was seen entering the receiver through the minute tubes, and falling in drops from them.

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* These experiments were performed in the presence of the Society.

The effect of this minute ductility was next tried with regard to glass rods of different shapes, which led to very curious results; specimens of which I beg now to lay before the Society, and to the most particular of these I shall take the liberty of directing their attention.

1. The specimen marked A was drawn from a narrow piece of window glass, cut with a diamond, and of course presenting very sharp angles; shown in figs. 1 and 4. This thread, when examined with a powerful microscope, was found to present a flatted oblong appearance with four well marked right angles, fig. 1. It is very likely that this peculiar shape is the cause of the superior lustre of the specimens of thread drawn from window glass; the round crystal rod always gives a dull appearance, and the lustre brightens as the specimens assume more an angular form. Fig. 7 is a square piece of crystal, the threads square.

2. The specimen marked B was drawn from a twisted piece of square glass, fig. 9. When examined with the glass, the thread was found to be square, but had lost the twisted appearance of the original.

3. The specimen marked C was drawn from a piece of fluted crystal, presenting four grooves, figs. 2 and 3. The fluted appearance is most distinctly retained by the spun glass, when placed before the microscope, fig. 2.

4. The specimen marked D was drawn from a twisted piece of grooved glass, see fig. 5. The threads retain the same form, but from the number of the grooves, a powerful glass is required to examine it. The spun glass appears to have the grooves straight.

From these examples, and from more than 50 others which have been tried, it is proved that glass has the singular property of retaining the shape, although brought to the fluid state, and although drawn into threads at that high temperature; and if the external form remain unchanged, we are entitled to conclude, that the internal form and arrangement will follow the same law.

Some experiments were next tried by combining glass of different colours into one rod, and then spinning it out into threads as before, fig. 8. The thread was always found to retain all the colours of the original rod unchanged, and did not present the smallest appearance of a break off in any of the colours, or of the slightest intermixture; sometimes 2, 3, and even 10 shades of colour were employed at once. These circumstances may open up an extensive field for investigation to those philosophers who delight in speculations regarding the ultimate atoms of bodies, and their peculiar shape. In the whole of these examples of ductility we find that the atoms of the glass have a tendency to retain their original form although its magnitude be diminished; the square, the oblong, the circular, the fluted and hollow rods, were still in the soft and silky threads to which they were spun, of the same shape as at first. Can the shape of the atoms, or any modification of the power of attraction, give rise to this? It is evident that the same portion which occupies the angles of the large piece of window glass will be extended over the angles of the spun threads; and the same is illustrated in another point of view by the many coloured glass rod, the shades of which retained their order and distinctive character.

The last experiments were with glass rods of different colours; the most of the colours appeared to have faded by the operation, particularly the yellow, which in some trials was nearly gone; the black became brown, and the purple and green were somewhat altered; the blue seemed to suffer no change. The white glass, coloured with arsenic, was very brittle.

The most of these specimens of spun glass are remarkably soft, like silk, and can be easily rolled up in the manner of common thread, and platted into ornaments. To the feel they resemble the hair of the head; that spun from black glass has often been mistaken for brown hair; it resembles the hair in another respect, for it retains the curls communicated to it by rolling it round a hot iron.

Note.—The letters A, B, C, D, in this paper, refer to the specimens which were handed round the Society, for the inspection of the members.

ARTICLE IX.

A Proposal for impelling Steam Vessels by horizontal Motion instead of Circular. By W. Ritchie, AM. Rector of the Royal Academy at Tain.

(To the Editor of the *Annals of Philosophy.*)

SIR,

If you consider the following speculations worthy of a place in the *Annals of Philosophy*, by inserting them you will very much oblige, Sir, your obedient servant,

WILLIAM RITCHIE.

In the application of steam to the impelling of boats and other vessels, the following requisites seem still wanting: In the first place, to apply the whole force in the direction in which the vessel moves, and in such a manner as not to increase the breadth of the vessel, which greatly retards its velocity. In the second place, to apply it in such a manner as not to interrupt the motion when it is found advantageous to employ sails either with or without the assistance of steam. And lastly, to arrange the moving power in such a manner as not to injure the sides of canals. In considering this important subject the following method occurred to me, which appears at least worthy of the attention of those who are engaged in building steam vessels. Instead of circular motion, let a horizontal motion be communicated to two rods passing through circular apertures in the stern of the vessel. To the end of each of these rods, two metallic plates of a convenient size are to be fixed, having their planes at right angles to the horizon, and moveable about strong well-polished joints. When the plates are shut they form a small angle with one another, and when opened to their utmost extent, they form about a right angle. When the rods are pushed suddenly out, the valves open and present a large surface to the reaction of the water, which will evidently push the vessel in the opposite direction; when the rod is drawn back, the valves shut, and present only a small surface to retard the motion of the vessel.

Not satisfied with reasoning alone, I had recourse to actual experiment. Having procured a long pole, I attached to one end of it a pair of valves similar to what I have described, and endeavoured to impel a boat by the strength of a man. The success of the experiment exceeded my most sanguine expectations. We moved with a velocity nearly equal to what could be produced by a man with a pair of oars, and it appeared obvious, that with a little practice, and a pair of such valves, the velocity could be greatly increased; but whether a similar effect could be produced on a large scale, is a question which cannot well be solved by calculation. Experiment alone must determine the point.

Should this mode be found to answer, a steam boat can be constructed with the same external appearance as any other vessel. Such a contrivance would be well adapted for boats moving on canals. It might also be applied with considerable advantage to vessels meeting with calms or contrary winds.

ARTICLE X.

Some Improvements of Lamps. By James Smithson, Esq. FRS.

(To the Editor of the *Annals of Philosophy*.)

SIR,

It is, I think, to be regretted, that those who cultivate science frequently withhold improvements in their apparatus and processes, from which they themselves derive advantage, owing to their not deeming them of sufficient magnitude for publication. When the sole view is to further a pursuit of whose importance to mankind a conviction exists, all that can do so should be imparted, however small may appear the merit which attaches to it.

Of the Wicks of Lamps.—The great length of wick commonly put to lamps for the purpose of supplying the part which combustion destroys, is, on several accounts, extremely inconvenient. It occupies much space in the vessel, and requires an enlargement of its capacity; it is frequently the occasion of much dirt, &c. This great length of wick is totally unnecessary.

Fig. 1.

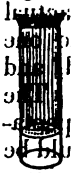


Fig. 2.

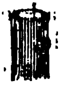


Fig. 3.

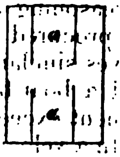
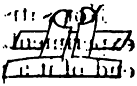


Fig. 4.



It is advantageously supplied by a tube containing a bit of cotton wick about its own length, or some cotton wool, fig. 1, and at the end of which is placed a stout bit of wick or cotton wool, fig. 2.

This loose end receives a supply of oil from the cotton under it with which it is put into contact, and when it becomes burned, it is easily renewed.

A loose ring of wick may in like manner be applied to the argand lamp. This removes the necessity of the long tube into which the wicks, now used, descend, and thus greatly contracts this lamp in height.

Of Wax Lamps.—Oil is a disagreeable combustible for small experimental purposes, and more especially when lamps are to be carried in travelling. I have, therefore, substituted wax for it. I experienced, however, at first, some difficulty in accomplishing my object.

The wicks of my lamps are a single cotton thread, waxed by drawing through melted wax. This wick is placed in a burner made of a bit of tinned iron sheet, cut like fig. 3, and the two parts *a a* raised into fig. 4.

This burner is placed in a china cup, about 1·65 inches in diameter, and 0·6 in. deep. Fragments of wax are pressed into this cup. But great care must be taken that each time the lamp is lighted, bits of wax are heaped up in contact with the wick, so that the flame shall immediately obtain a supply of melted wax. This is the great secret on which the burning of wax lamps depends.

When the wick is consumed, the wax must be pierced with a large pin down to the burner, and a fresh bit of waxed cotton introduced.

I employ a wax lamp for the blowpipe. This has, of course, a much larger wick, and this wick has a detached end to it, as above described.

Extinguishing Lamps.—The best way of doing this is to extinguish the ignited part of the wick by putting sound wax on to it, and then blowing the flame out. This preserves the wick entire for future lighting again.

This mode applied to candles is much preferable to the use of an extinguisher, or douters, to which there are many objections.

Article XI.

On Works in Niello and the Pirotechnia of Venoceio Biringuccio Siennese. By the Rev. J. J. Conybeare.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Bath Easton, Oct. 14, 1822.*

In Mr. Ottley's interesting and learned History of Engraving, vol. i. pp. 262 and 270, two accounts are given of the process used in the execution of the ornamental work termed Niello: the former, very short, and evidently inaccurate, from Vasari; the latter from a modern virtuoso (the Count Seratti), whose statement, although more correct, is unsupported by any reference to earlier authorities, not to mention that Seratti himself is (as Mr. Ottley with justice remarks) somewhat wanting both in accuracy and in judgment. It is sufficiently known that the Niello (independently of the esteem in which it was once held, and the real merits and beauty of the works executed in it by Finiguerra and others) has been yet more ennobled by having given birth to the invaluable art of transferring impressions from engraved plates to paper. The following description, therefore, of the mode which seems to have been usually adopted for the composition of the enamel (if we may so call it), as well as for its insertion into the cavities produced by the graver, may not, perhaps, be unacceptable to your readers. It has the merit of coming from an author who lived before the art was yet obsolete, and who seems himself to have been a practical man of considerable intelligence for his day.

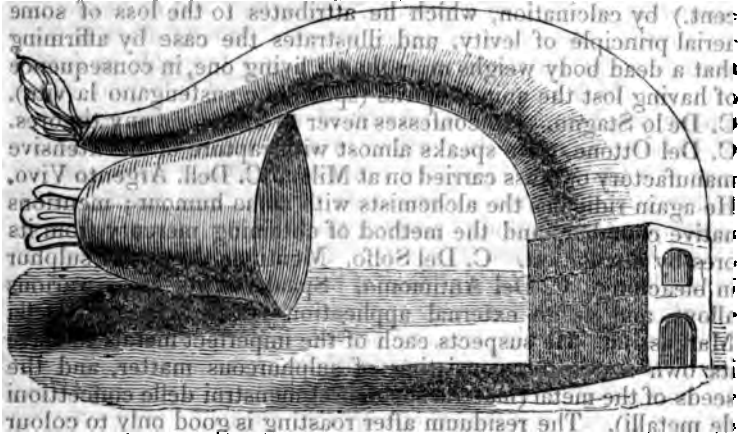
"The Niello," he informs us, "is composed by taking one part of pure silver, two of copper, and three of pure lead, which must be fused together, and in that state poured into a long-necked earthenware matrass, half filled with levigated sulphur, the mouth of the vessel is immediately to be closed, and the contents left to cool. The mass which results, when levigated and washed, is ready for the purposes of the artist. The cavities made by the *burin* having been filled with it, the plate is to be held over a small furnace fed with a mixture of charcoal and wood, taking care to distribute the enamel carefully with a proper instrument. As soon as its fusion has taken place, the plate is to be removed, and when sufficiently cooled is to be cleared by the file, and polished by fine pumice and tripoli."

To the four ingredients here enumerated, the receipt given by Seratti adds a fifth, borax, the use of which is not immediately apparent. A small portion might, perhaps, be put into the crucible containing the alloy, to cover it, and facilitate its fusion, but it could scarcely enter into the composition of the enamel itself.

The *Pirotechnia* of *Biringuccio*, from which the above is extracted, is a book of somewhat rare occurrence, and, for that reason, perhaps, has not been noticed as it deserves by those who have employed themselves in tracing the progress of mineralogy and metallurgy. It was first printed at Venice in the year 1540, and, therefore, preceded by 20 years the more splendid volume of G. Agricola, which on subjects immediately connected with mining is unquestionably more copious and instructive. Biringuccio, however, embraces a much wider range, and his work is certainly better calculated to illustrate the state of knowledge at the era of its composition than that of his German successor. The good Italian too manifests, if not so much of erudition, a far more lively play of the imagination. His work is divided into 10 books. The first treats of metals; the second, of semimetals, with some earthy and saline substances; the third, on the assay and reduction of metallic substances; the fourth, on the assay and refining chiefly *viâ humidâ*; the fifth, on alloys; the sixth, seventh, and eighth, on the art of casting metals, treating largely on all that concerns bell and cannon founderies; the ninth, on distillation; on the arts of the workers in gold, copper, iron and tin-wiredrawing, gilding; the manufacture of metallic specula; of crucibles, of pottery, and of mortar; the tenth, on nitre, gunpowder, artillery, and fireworks. This abridged table of contents will suffice to give a general notion of the work. I subjoin a brief statement of such passages as, on a cursory perusal, appeared interesting:

B. 1. Under the chapter "Luoghi de la Miniera," he speaks slightingly of the "charlatanerie" of those who pretend to discover mines by any other than natural indices; his directions, as far as they go, are very sensible. He mentions the custom of baptizing or dedicating the mine by the name of the deity, or of some patron saint. He recommends the driving an adit from the bottom or side of the hill in preference to the older usage of digging downwards from the point where the ore comes to *day* (algiorno). He mentions a productive mine of copper and lead below Inspruck. C. Dell'Oro. He opposes strongly the dreams and impositions of the alchemists. C. Dell, Argento. Quotes G. Agricola (quere, from what work?) as relating the discovery of a mass of silver ore, in one of the Saxon mines, sufficiently large to make a table and a seat, or stool (tripode). He seems to have been acquainted with the red and grey silver ores, and with the usual modes of roasting and reducing them. C. Del Rame. Italy is in this metal "richissima;" mentions the peacock and grey copper ores, especially the richness of the latter. C. del Piombo. He notices its acquiring weight (from 8 to 10 per cent.) by calcination, which he attributes to the loss of some aerial principle of levity, and illustrates the case by affirming that a dead body weighs more than a living one, in consequence of having lost the animal spirits (spiriti che sustengano la vita). C. De lo Stagno. He confesses never to have seen any tin ores. C. Del Ottone. He speaks almost with rapture of an extensive manufactory of brass carried on at Milan. C. Dell. Argento Vivo. He again ridicules the alchemists with some humour; mentions native cinnabar, and the method of obtaining mercury from its ores by distillation. C. Del Solfo. Mentions the use of sulphur in bleaching. C. Del Antimonio. Speaks of its use in various alloys, and as an external application in medicine. C. Della Margassita. He suspects each of the imperfect metals to have its own marcasite, consisting of sulphureous matter, and the seeds of the metal (materia seconde et menstrui delle concettioni de metalli). The residuum after roasting is good only to colour porcelain or glass, and to cheat the alchemists. Argues against its being entirely "fumosita," (a substance capable of sublimation?) but appears to entertain the belief of his age, that mineral veins grow like organized bodies. C. Del Vetriolo. Describes the manufactory of Roman vitriol, the strongest form of which "non Vetriolo ma Cuperosa si chiama." C. Dell Alume di Roccha. Gives a detailed and practical account of its manufactory. Mentions the district of La Tolfa as not likely to be exhausted before "l'ultimo giorno del mondo." C. Del Arsenico, Orpimento et Risagallo. Mentions the alloys of arsenic with copper, brass, and lead. Its ores come from the Hellespont and Cappadocia. Notices the observation of "li prattici minerals, that arsenic is mixed with almost all metallic ores, and the opinion that in volatilizing it carries off whatever silver they may contain. C. Della Giallamina, Zaffara, et Manganese. This chapter contains the earliest mention with which I am acquainted of manganese. Its use, both in tinging porcelain and glass, and in rendering the latter colourless, is noticed. The chapters on gems and glass contain little of interest. Those on the assay and reduction of metals are entirely practical, and show an intimate acquaintance with the detail of all the processes then in use. In treating of alloys, he mentions the superiority of English tin.

The alloy for bell metal he states to contain from 22 to 26 per cent. of tin; that for other purposes of casting from 8 to 12. Enters largely into detail on the casting both of artillery and bells. At p. 100, he mentions a singular mode of soldering large bells when damaged, by carrying the curved chimney of a furnace constructed for the purpose in the direction of the fissure, and cementing the edges thus softened by the addition of melted bronze. This is, I suspect, a process never adopted in our bell founderies.



Of the value of his further directions for casting, &c. I am not competent to judge; they appear tolerably full. At p. 109, he mentions, that in the manufactory of bronze, lead was occasionally substituted *in part* for tin, as being cheaper. C. del Far le Palle di Ferro. He states that cannon balls of cast-iron were first used in Italy, by Charles, King of France, in his attack on Naples, A. D. 1495. He mentions that some added antimony, others copper, and others arsenic, with the intent of rendering the metal more fusible, but objects that it is rendered at the same time more brittle. C. di Formare Rilievi. He appears to have been acquainted with all the modes of casting and modelling [illegible] He much praises the ingenuity of a Sienese artist, G. B. Palori, who invented a new species of moulds for casting in plaister, by covering the original statue with a mask of paper, or rather *papier machae* (carta pasta) and linen, stratum superstratum. When this was sufficiently thick and hard, it was cut from the statue in convenient portions; then reunited, strengthened by the addition of fresh matter, and rendered impermeable to water by wax and asphaltum (pece greca). The moulds thus produced, were light, portable, unexpensive, not liable to break, and well adapted for their object. C. del Arte Alchimica, and C. del Arte Distillatoria. In the former of these, he again attacks the alchemists as to the probable attainment of their object, but allows that in their researches they frequently made discoveries of great interest and value. The latter contains nothing which at the present day could inform or interest the chemical reader.

C. Del Arte del Fabro Orefice. Besides the article already quoted on the Niello, contains directions for soldering, tempering, and colouring gold, and for enamelling, but nothing on the composition of the enamels or pastes themselves. C. del Arte del Fabro Ramario. Mentions the art of tinning copper vessels. C. del Arte del Fabro Ferrario. Treats of the manufactory and tempering of steel, of colouring, engraving, and damasquining its surface; these arts he terms *secrets*. Among other of these secrets is one for rendering iron *soft*, and *tractable as lead*, a process which must have been in request at a period which, among other works of art, produced many beautiful specimens of chasing in iron. It consists in exposing to the continued heat of a furnace the iron first anointed with *oil of bitter almonds*, and then coated with a paste made of wax, *assa fœtida*, and a small quantity of alkali, covering the whole with a strong lute. C. del Arte del Fabro Stagnario. The composition for printer's types, he states to be six parts of fine tin, one of lead, and one of antimony.

In subsequent chapters, he describes the process of recovering gold and silver from plated articles, or mineral compounds, by amalgamation with mercury. For this secret he states himself to have given a diamond ring worth 25 ducats.

A more interesting chapter is that on the "Pratica et Modo di fare li Specchi di Metallo." He mentions a tradition as to the existence of telescopic specula, as far back, if I understand him, as the age of Augustus. "Che mostrano l'imagine delle cose lontane et non delle propinque." He treats also of burning specula, of one especially belonging to a German, by which gold was kept in a state of fusion. He mentions another (telescopic?) speculum, said to have existed at Tunis. "Il quale era tanto lucido, che del piu alto della Rocca voltan dolo verso il Porto della Goleta vi si discernava tutti le navi che varano surte, et tutte le genti che arano con esse, et de che colori et habiti eran vestiti: certo credo che fusse con questi trovata la prospettiva pratica di Pittori et la ragioni d'essa." (Or was this a diminishing mirror?) His *secret* for the composition of metallic mirrors is, "three parts tin, and one copper. Upon this alloy, when in fusion, throw (for every pound) one ounce of tartar, and half an ounce of arsenic."

The chapters on the art of pottery, and making lime, appear to contain nothing remarkable. The same may, perhaps, be said of the chapters relating to artillery and fireworks; but with these subjects, I have no acquaintance.

The concluding chapter is perfectly characteristic of the Italian. "Del Fuocho che consuma et non fa cenere, et e potente pui che altro fuoco, del quale ne e Fabro el gran Figliol di Venere."

"Chealtro dir non virole che cupido."

Upon the whole, although this scarce volume from its meagreness and imperfections forms a singular contrast to the bulk and fullness of detail which might be expected in a metallurgical encyclopædia of the present day, it is unquestionably for its age a work of no common merit and interest; and the author is fairly entitled, from his practical intelligence and industry, to be ranked among those who contributed to realize the almost prophetic verse of his immortal countryman:

"Esperienza
Ch' Esser suol fonte a i rivi di nostri arti."—(Dante Paradiso.)

I subjoin the account of the manufactory of Niello from the original: "Niellasi ancora per ornamento de lavori certi intagli o profili et questo prima si compone pigliando una parte di argento fino, due di rame, et tre di piombo fino, et in un vaso di terra che habbi el collo stretto et longo sempee la meta di solfo macinato, et sopravi si gitta fusi gli detti metalli, et con terra subito messi si chuida la boccha del vaso, et benissimo si rimena. Dipdoi freddo rompendo il vaso se ne cava et netta, et lavasi et alfin si macina, et adoperasi riempiendo li vacui de lavori che s'vuole, et a un fornelletto fatto di carboni grossi con alquanto di fiamme di legna et con uno mantachetto soffiandovi dentro savoiva et si fa sopra al lavoro vostro scorrere collocandolo alquanto con uno legnetto o ferro quando e scorso, et si cava et lassa freddare. Dipoi cosi fatto con una lima levando el superfluo si senopre, et con una poca di camra et pomice sottile si pulisce, et con la terra di tripoli fregandolo si fa lucido et bello."—(P. 135.)

The Pirotechnia* was a second time printed at Venice, A. D. 1550, with the original wood-cuts, and some alterations in the orthography. A French translation appears to have been printed at Paris, A. D. 1572. The author is spoken of with commendation both by Agricola (in his preface), and Cardan (De Subtil). The former terms him an eloquent writer, which is true to a certain extent, though his eloquence, in spite of his origin, be indeed of the purest Tuscan. I had almost forgotten to s that Venoceio Biringuccio was a native of Sienna.

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* See also Brunet's Dict. Bibliographique, article Biringuccio.

I am, dear Sir, yours truly,
J. J. CONYBEAR

ARTICLE XII.

ANALYSES OF BOOKS.

Philosophical Transactions of the Royal Society of London 1822. Part I.

We have to apologize for so long delaying to analyze this which contains a series of papers of great importance; tw the most interesting, however, have already been inserte length in the three preceding numbers of the *Annals.*

I. *The Bakerian Lecture.—An Account of Experimen determine the Amount of the Dip of the Magnetic Needle in don, in August* 1821; *with Remarks on the Instruments whic usually employed in such Determinations.* By Capt. Ed Sabine, of the Royal Regiment of Artillery, FRS.

Capt. Sabine remarks, that the increased attention whicl been given of late years to the subject of magnetism, and consequent advance that has been made in the science, re it desirable that a greater degree of accuracy should be obta in the observation of its terrestrial phenomena, than has hitl been the case. The instruments for ascertaining the dip o needle, it is stated, have received little or no improvement ing the last 50 years, and produce results, which, with e precaution, can be considered as approximate only.

After describing the imperfections in the instruments all to, and explaining the errors which originate in them, the au proceeds to give an account of a dipping-needle, whic requested Mr. Dollond to make, on a construction suggest Prof. Meyer, of Gottingen; as well as of the mode of obs tion therewith. "The needle is a parallelopipedon of el inches and a half in length, four-tenths in breadth, and twentieth in thickness; the ends are rounded; and a line ma on the face of the needle passing through the centre tc extremities, answers the purpose of an index. The cylin axis on which the needle revolves, is of bell metal, termin where it rests on the agate planes, by cylinders of less diam the finer these terminations are made, so long as they d bend with the weight of the needle, the more accurate w the oscillations; small grooves in the thicker part of the receive the Y's, which raise and lower the needle on its supports, and ensure that the same parts of the axis rest in each observation on the planes. A small brass sphere traverses on a steel screw, inserted in the lower edge of the needle, as nearly as possible in the perpendicular to the index line passing through the axis of motion; by this mechanism, the centre of gravity of the needle, with the screw and sphere, may be made to fall more or less below the axis of motion, according as the sphere is screwed nearer or more distant from the needle, and according as spheres of greater or less diameter are employed. The object proposed in thus separating the centres of motion and gravity, is to give the needle a force arising from its own weight to assist that of magnetism in overcoming the inequalities of the axis, and thus to cause the needle to return, after oscillation, with more certainty to the same point of the divided limb than it would do were the centres strictly coincident."

"The centres of motion and of gravity not coinciding, the position which the needle assumes, when placed in the magnetic meridian, is not that of the dip: but the dip is deducible, by an easy calculation, from observations made with such a needle, according to the following directions:

"If the needle has been carefully made, and the screw inserted truly as described," "two observations made in the magnetic meridian are sufficient for the determination of the dip, the two faces of the needle being successively towards the observers, renewing the position of the axis on its supports in such a manner that the edge of the needle which is uppermost in the one observation becomes lowermost in the other; the angles which the needle makes with the vertical in these two positions being read, the mean of the tangents of those angles is the co-tangent of the dip. But when needles are used in which this adjustment has not been made, or where its accuracy cannot be relied on, four observations are required; two being those which are already directed; the two others are similar to them, but with the poles of the needle reversed; calling then the first arcs F and f, and those with the poles reversed G and g, and taking

$$\text{tang. } F + \text{tan. } f = A$$
$$\text{tang. } F - \text{tan. } f = B$$
$$\text{tang. } G + \text{tan. } g = C$$
$$\text{tang. } G - \text{tan. } g = D$$

$\frac{A \,.\, D}{B+D} + \frac{B \,.\, C}{B+D}$ = twice the co-tangent of the dip."

"The instrument in which the needle was tried is already described in the Philosophical Transactions for 1819, p. 132, and several improvements which have since been added, in the Appendix to Capt. Parry's Voyages of Discovery, pp. 107, 139, &c."

"The experiments were made in the nursery-garden in the

Regent's Park, by permission of Mr. Jenkins, the proprietor. The situation is in all respects an eligible one, being far removed from the neighbourhood of iron."

"The results by three different methods collected into one view, are as follow, viz.

By 10 experiments with Meyer's needle	70° 02·9
By the times of oscillation in the magnetic meridian, and in the plane perpendicular to it; mean by three needles	70 04·0
By the times of vertical and horizontal oscillation.	70 02·6

"Whence 70° 03′ may be considered as the mean dip of the needle towards the north in the Regent's Park, in August and September, 1821, within four hours of noon, being the limit within which all the experiments were made."

As the observations of Mr. Nairne in 1772, and of Mr. Cavendish in 1776, give an approximation of 72° 25′ for the dip in 1774, we obtain, it is stated, 3·02′ as the mean annual rate of diminution between 1774 and 1821; and if we take Mr. Whiston's determination of the dip in 1720, 75° 10′, we obtain between the years 1720 and 1774, an annual diminution of 3·05′.

Capt. Sabine says, "in conclusion, there appears reason to presume, from the preceding experiments, that the dip itself may be determined by Meyer's needle within a much smaller limit of uncertainty than has hitherto been the case by needles of the usual construction."

II. *Some Positions respecting the Influence of the Voltaic Battery in obviating the Effects of the Division of the Eighth Pair of Nerves.* Drawn up by A. P. Wilson Philip, MD. FRS. Edin. (Communicated by B. C. Brodie, Esq. FRS.)

This short paper appears to establish two momentous and novel facts in physiology; we shall give the principal results of Dr. Philip's investigation in his own words, distinguishing the important circumstances by the *italic* character.

"In some experiments in which the nerves of the eighth pair were divided in the neck of a rabbit, *and the ends not displaced*, and the animal was allowed to live some hours, it was found that food swallowed immediately before the division of the nerves, *was considerably digested*, even when the divided ends of the nerves had retracted to the distance of a quarter of an inch from each other."

"In other experiments in which, after the division of the nerves, *the divided ends had been turned completely away from each other, little or no perfectly digested food, when the animal was allowed to live some hours, was found in the stomach*; and the longer the animal lived, the smaller was the proportion of digested food found in the stomach; the great mass having the appearance of masticated food, which was not sensibly lessened in quantity, however long the animal lived. In an experiment in which, *under such circumstances*, the stomach was exposed, from the time of the division of the nerves, to the influence of a voltaic battery sent through the lower portion of the divided nerves, *its contents were apparently as much changed as they would have been in the same time in the healthy animal.* The change was also of the same kind, the contents of the stomach assuming a dark colour, and those of the pyloric end being more uniform, and of a firmer consistence than those of the central and cardiac portions of the stomach; while the whole contents became less in quantity." . . . .

III. *On some Alvine Concretions found in the Colon of a Young Man in Lancashire, after Death.* By J. G. Children, Esq. FRS. &c. &c. (Communicated by the Society for Promoting Animal Chemistry.)

An abstract of this paper has already been given in the *Annals* for January last, p. 75, but we may add the following particulars: The concretions were found lodged in the arch of the colon, the coats of which were much thickened and formed into a sort of pouch, where they lay. The peritoneum was but little inflamed, the other viscera were healthy. The unfortunate subject of the paper never took a single repast without oatmeal in some shape or other, and the concretions consist of alternating concentric layers of a velvety fibrous substance from the inner coat enveloping the farina of the oat, and of phosphate of lime, together with the ammoniaco-magnesian phosphate.

IV. *On the Concentric Adjustment of a Triple Object Glass.* By William Hyde Wollaston, MD. VPRS.

Dr. Wollaston here describes a method of correcting the central adjustment of a triple object-glass which appears not to have been used for that purpose, but for the details of which we must refer our readers to the paper itself, as they would be useless without the accompanying plate. The principle and its result are explained as follows:

"When any bright object is viewed through a glass of this construction, without an eye-glass, there may be observed at the same time with the refracted image, a series of fainter images, that are found by two reflections from the different surfaces; and as the position of each of these images is dependent on the curvatures of that pair of surfaces by which it is formed, they appear at different distances from the object glass. Since the number of surfaces is six, the number of binary combinations of these surfaces is 15; and just so many images formed by reflection may be discerned. It is manifest, that if the glasses be duly adjusted to each other, so that their axes are correctly coincident, then this series of images must be all situated in the same straight line; and conversely, that any defective position may be immediately detected by a derangement of the line of images."

"By these guides alone, I have now so repeatedly restored my object-glass to correct performance after having removed it from its cell [in a telescope of 45 inches focus, made by Dollond in 1771], that I may venture, with considerable confidence, to recommend trial of the method to those who wish to perfect glasses of this construction."

V. *On a new Species of Rhinoceros found in the Interior of Africa, the Skull of which bears a close Resemblance to that found in a Fossil State in Siberia and other Countries.* By Sir Everard Home, Bart. VPRS.

"It has been hitherto asserted," we are informed in the commencement of this paper, "as one of the most curious circumstances in the history of the earth, that all the bones that are found in a fossil state, differ from those belonging to animals now in existence; and I believe that this is generally admitted, and that there is no fact upon record, by which it has been absolutely contradicted; but the observations I am about to state respecting this rhinoceros, illustrated by the drawings that accompany them, will go a great way to stagger our belief upon this subject."

"The skull of the animal belonging to this new species of rhinoceros, now living in Africa, was brought to this country by Mr. Campbell, one of the missionaries sent there from the London Missionary Society, and is deposited in their Museum in the Old Jewry."

Sir Everard then proceeds to give, from Mr. Campbell's memoranda, an account of the locality and habits of the animal, but as the substance of these has already appeared in various publications, we shall pass to the description of the skull. This is shown, with the assistance of two engravings, "to bear so close a resemblance to the fossil skulls from Siberia, as to leave no prominent characteristic mark between them;" whence the author is led to believe, "that although many animals belonging to former ages may be extinct, they are not necessarily so: no change having taken place in our globe, which had destroyed all existing animals, and, therefore, many of them may be actually in being, although we have not been able to discover them." After arguing from the existence in Africa of immense tracts of country yet unexplored, that "we have no right to assume that large animals, although not met with, do not exist;" he gives the following particulars of the migration of an animal of another kind, as explaining "in what way particular animals may elude our inquiry at one time, and at another be brought within our reach."

"Mr. Campbell says, he found that the wild ass or quagga, migrates in winter from the tropics to the vicinity of the Malaleveen river, which, though further to the south, is reported to be warmer than within the tropic of Capricorn, when the sun has retired to the northern hemisphere. He saw bands of 200 or 300, all travelling south, when on his return from the vicinity of the tropic; and various Bushmen, as he proceeded south, inquired if the quaggas were coming. Their stay lasts from two to three months, which in that part of Africa is called the Bushmen's harvest. The lions who follow them are the chief butchers. During that season, the first thing a Bushman does on awaking is, to look to the heavens to discover vultures hovering at an immense height; under any of them he is sure to find a quagga that had been slain by a lion in the night."

These are succeeded by observations on the docile and tameable character of the elephant, and on the savage and stupid nature of the rhinoceros, which are followed by some inferences respecting the latter subject, from the diminutive cavity of the cranium, and consequent smallness of the cerebrum in the last-mentioned animal. An account of the manners and habits of the Asiatic rhinoceros, kept for three years in the menagerie at Exeter Change, is subjoined; and the paper concludes as follows:

"The account in the Bible of an unicorn not to be tamed, mentioned by Job, has so great an affinity to this animal, that there is much reason to believe that it is the same, more especially as no other animal has ever been described so devoid of intellect. In that age, the short horn might readily be overlooked, as it cannot be considered as an offensive weapon; and the smoothness of the animal's skin would give it a greater resemblance to the horse than to any other animal."

VI. *Extract of a Letter from Capt. Basil Hall, RN. FRS. to William Hyde Wollaston, MD. FRS. containing Observations of a Comet seen at Valparaiso.*

VII. *Elements of Capt. Hall's Comet.* By J. Brinkley, DD. FRS. and MRIA. and Andrew's Professor of Astronomy in the University of Dublin. (In a Letter addressed to Dr. Wollaston.)

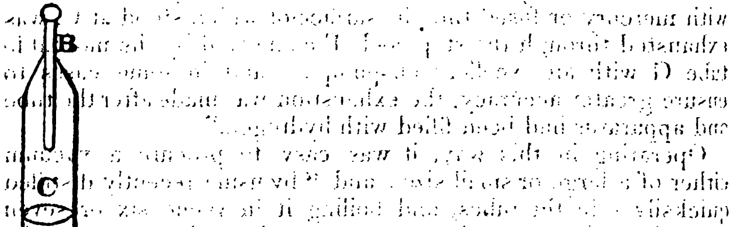
This comet, which had been seen by astronomers in Europe, before it passed its perihelion, remained visible at Valparaiso for 33 days, and Capt. Hall has furnished a valuable set of observations on it, from which Dr. Brinkley has deduced its elements by an improved mode of calculation. On April 8, 1821, it was distant nearly 1·41 from the earth, the sun's distance from the earth being unity, and on May 3, when last seen, about 2·64. It is interesting to astronomers on account of its small perihelion distance; out of 116 comets in Delambre's Catalogue, the orbits of which have been computed, there are only three that pass nearer the sun. In this, as well as in its great inclination, this comet agrees with that observed in 1593, whence it is probable that they are the same. Some sketches of it by Capt. Hall are annexed to his letter in a plate.

VIII. *On the Electrical Phenomena exhibited in vacuo.* By Sir Humphry Davy, Bart. PRS.

It is remarked in the commencement of this highly interesting paper, that the relations of electricity to heat, light, and chemical attractions, together with the discovery of its connexion with magnetism, have opened an extensive field of inquiry in physical science, and have imparted much additional interest to electrical investigations.

"Is electricity a subtile elastic fluid? Or are electrical effects merely the exhibition of the attractive powers of the particles of bodies? Are heat and light elements of electricity, or merely the effects of its action? Is magnetism identical with electricity, or an independent agent put in motion or activity by electricity?"

The solution of these queries, it is observed, "is of the highest importance; and though some persons have undertaken to answer them in the most positive manner," yet there are few sagacious reasoners who think that our present data are sufficient for decision in such abstruse parts of corpuscular philosophy.



"It appeared to me," continues Sir Humphry, "an object of considerable moment, and one intimately connected with all these queries, *the relations of electricity to space, as nearly void of matter as it can be made on the surface of the earth.*" Walsh and Morgan had concluded from their experiments, that the electrical light was not producible in a perfect torricellian vacuum; and the latter that such a vacuum likewise prevented the charging of coated glass; but it being well known that very rare vapour of mercury exists in the most perfect vacuum of that nature that can be made, Sir H. could not help doubting the perfect accuracy of these results, and "resolved not only to examine them experimentally, but likewise, by using a comparatively fixed metal in fusion for making the vacuum, to exclude, as far as was possible, the presence of any volatile matter."

The apparatus that he employed consisted of a curved glass tube A D, with one leg A closed, and longer than the other. In this closed leg, a wire of platinum B was hermetically cemented, for the purpose of transmitting the electricity; or to ascertain the power of the vacuum to receive a charge, a small cylinder of tin or platinum foil E was placed as a cap on tubes not having the wire B. The open end D, when the closed leg had been filled with mercury or fused tin, the surface of which stood at C, was exhausted through the stop-cock F connected by the moveable tube G with an excellent air-pump; "and in some cases to ensure greater accuracy, the exhaustion was made after the tube and apparatus had been filled with hydrogen."

Operating in this way, it was easy to procure a vacuum either of a large or small size: and "by using recently distilled quicksilver in the tubes, and boiling it in vacuo six or seven times from the top to the bottom, and from the bottom to the top, making it vibrate repeatedly by striking it with a small piece of wood, a column was obtained in the tube free from the smallest particle of air;" but vapour of mercury was sometimes produced, filling a minute globular space, to discover the cause of which gave the author a great deal of trouble.

"He found that in all cases when the mercurial vacuum was perfect, it was permeable to electricity, and was rendered luminous by either the common spark, or the shock from a Leyden jar, and the coated glass surrounding it became charged; but the degree of intensity of these phenomena depended upon the temperature. When the tube was very hot, the electric light appeared in the vapour of a bright-green colour, and of great density; as the temperature diminished, it lost its vividness; and when it was artificially cooled to 20° below zero of Fahr. it was so faint as to require considerable darkness to be perceptible." The change communicated to the metallic foil was likewise higher, the higher the temperature, which, like the other phenomenon, must depend upon the different density of the mercurial vapour.

"A very beautiful phenomenon occurred in boiling the mercury in the exhausted tube; which showed the great brilliancy of the electrical light in pure dense vapour of mercury. In the formation and condensation of the globules of mercurial vapour, the electricity produced by the friction of the mercury against the glass was discharged through the vapour with sparks so bright as to be visible in day-light.

When the minutest quantity of rare air was introduced into the mercurial vacuum, the colour of the electrical light changed from green to sea-green, and by increasing the quantity, to blue and purple; in low temperatures, the vacuum became a much better conductor.

The results were precisely the same, when a difficultly fusible amalgam of mercury and tin was used as when pure mercury was employed, and in a vacuum above fused tin, the same phenomena were also exhibited. Electrical and magnetic repulsions and attractions took place as they would have done in air. It was ascertained "that the feebleness of the light in the more perfect vacuum was not owing merely to a smaller quantity of electricity passing through it; for the same discharge which produced a faint green light in the upper part of the tube, produced a bright purple light in the lower part, and a strong spark in the atmosphere."

Pure olive oil and chloride of antimony were severally tried in the vacuum, and it was found "that the light produced by the electricity passing through the vapour of the chloride was much more brilliant than that produced by it in passing through the vapour of the oil; and in the last it was more brilliant than in the vapour of mercury at common temperatures: the lights were of different colours, being of a pure white in the vapour of the chloride, and of a red, inclined to purple, in that of the oil; and in both cases permanent elastic fluid was produced by its transmission."

Sir H. Davy observes, "The law of the diminution of the density of vapours by diminution of temperature has not been accurately ascertained; but I have no doubt, from the experiments of Mr. Dalton, and some I have made myself, that it is represented by a geometrical progression; the decrements of temperature being in arithmetical progression; and in three pure fluids that I operated upon (water, chloride of phosphorus, and sulphuret of carbon), the ratio seemed nearly uniform for the same number of degrees below the boiling point; and (taking intervals of 20 degrees of temperature) ·369416. Upon this datum, Sir Humphry was obliged to Mr. Babbage for the calculation, that considering the elastic force of vapour of water at 52° to be equal to raise by its pressure about ·45 of an inch of mercury; the relative strengths of vapour will be, reckoning the boiling points all from 52°, for mercury at 600°, 000015615; for oil at 540°, ·0016819; for chloride of antimony at 340°, 01692; and for tin at 5000°, 37015 preceded by 48 zeros. These numbers are given to show how minute the quantity of matter must be in vapours where its effects are distinct upon electrical phenomena, especially with respect to mercury artificially cooled, and in vapours from comparatively fixed substances.

The diminution of the temperature of the torricellian vacuum, to as low as about 20°, appeared to diminish its power of transmitting electricity; but between 20° above and 20° below zero, the lowest temperature that could be produced by pounded ice and muriate of lime, the power seemed stationary, and nearly the same as that of the vacuum above tin. "At all temperatures below 200°, the mercurial vacuum was a much worse conductor than highly rarefied air."

"It is evident from these general results," the author continues, "that the light (and probably the heat) generated in electrical discharges depends *principally* on some properties or substances belonging to the ponderable matter through which it passes; but they prove likewise that space, where there is no appreciable quantity of this matter, is capable of exhibiting electrical phenomena; and, under this point of view, they are favourable to the idea of the phenomena of electricity being produced by a highly subtile fluid or fluids, of which the particles are repulsive, with respect to each other, and attractive of the particles of other matter."

To this succeed some further observations on the nature of electrical phenomena and their relations, which are terminated by a remark, that the luminous appearances of electrical action must be considered as secondary, while the uniform exertions of attractions and repulsions, under all circumstances, point them out as primary and invariable phenomena of electricity. This valuable communication is then concluded by the important statement, that recently distilled mercury which has been afterwards boiled and cooled in the atmosphere, *and which presents a perfectly smooth surface in a barometer tube*, emits air when strongly heated in vacuo; and an instance is given in which the metal was observed to imbibe air.

IX. *Croonian Lecture.—On the Anatomical Structure of the Eye; illustrated by Microscopical Drawings, executed by F. Bauer, Esq.* By Sir Everard Home, Bart. VPRS.

The contents of this lecture would be unintelligible without the engravings, in which the structure of the visual organ is minutely and beautifully delineated.

X. *A Letter from John Pond, Esq. Astronomer Royal, to Sir H. Davy, Bart, PRS. relative to a Derangement in the Mural Circle at the Royal Observatory.*

As the amount of error occasioned by this derangement has been stated by Mr. Pond in the Preface to the Greenwich Observations for 1820; and as the derangement itself has been rectified by Mr. Troughton, it is unnecessary to abridge this letter.

XI. *On the Finite Extent of the Atmosphere.* By Dr. Wollaston, VPRS.

This admirable paper is printed entire in the last number of the *Annals.**

XII. *On the Expansion in a Series of the Attraction of a Spheroid.* By James Ivory, MA. FRS.

This elaborate paper does not admit of profitable abridgment. The author suggests, in the conclusion, that Laplace's theory of the figure of the planets "will probably be found to hinge on this proposition, that a spheroid, whether homogeneous or heterogeneous, cannot be in equilibrium by means of a rotatory motion about an axis, and the joint effect of the attraction of its own particles, and of the other bodies in the system, unless its radius be a function of three rectangular co-ordinates."

XIII. *On the late extraordinary Depression of the Barometer.* By Luke Howard, Esq. FRS.

Mr. Howard has already stated the amount of this remarkable depression in the *Annals* for February last, p. 160. Among other observations respecting it in this paper, which is illustrated by a plate of the autographic curve of the barometric variations, are the following: "It will be seen that this great depression was preceded by abrupt changes, fluctuating for 30 days, chiefly between 29·5 and 30 inches, during a continuance of stormy weather; and that the depression itself was 14 or 15 days in progress from the point of 30 inches, to that from which it finally rose in three days. The rain for these two months is 10·10 inches, a quantity without precedent in the same space of time at London; that is to say, without one on record."

XIV. *On the anomalous Magnetic Action of Hot Iron between the White and Blood-red Heat.* By Peter Barlow, Esq. of the Royal Military Academy. (Communicated by Major Thomas Colby, of the Royal Engineers, FRS.)

Certain theoretical results relative to the magnetic action of iron, obtained by Mr. Bonnycastle, induced Mr. Barlow to ascertain the relative attraction which different species of iron and steel had for the magnet. The following are the results of his experiments for that purpose, assuming the tangents of the angles formed by the deviation of the needle when acted upon by equal-sized bars of the several descriptions of metal placed in the direction of the dip, as the measure of the disturbing power.

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* The following investigation of the same subject by a method entirely different from Dr. Wollaston's, has been pointed out in a contemporary Journal:

"The highest portions of the atmosphere, which is carried round in 23 hours and 56 minutes, by the rotation of the earth about its axis, would be projected into space, if their centrifugal force at that distance were not less than their gravitation towards the centre. But the centrifugal force is directly as the distance, while the power of gravity is as its square. Consequently when the centrifugal force at the distance of 6·6 radii of the earth is augmented as many times, the corresponding gravitation is diminished by its square, or 43·7 times, their relative proportion being thus changed to 289. Now the centrifugal force being only the 289th part of gravity at the surface of the equator, it will, therefore, just balance this power at the distance of 6·6 radii from the centre, or at the elevation of 22,200 miles."—(Leslie on Meteorology, Supplement to the Encyclopædia Britannica, vol. v. p. 325.)

	Magnetic power.
Malleable iron	100
Cast iron	48
Blistered steel, soft	67
Blistered steel, hard	53
Shear steel, soft	66
Shear steel, hard	53
Cast steel, soft	74
Cast steel, hard	49

It being obvious from these experiments that the intensity of the magnetic power was in proportion to the softness of the metal, the author became desirous of determining the magnetic relations of each variety when rendered perfectly soft by being heated in a furnace. With this view, bars of each substance, of equal size, were rendered white-hot, when it was found that their powers, as was anticipated, agreed nearly with each other.

"While carrying on these experiments," says Mr. Barlow, "it had been observed, both by Mr. Bonnycastle and myself, that between the white heat of the metal, when all magnetic action was lost, and the blood-red heat, at which it was the strongest, there was an intermediate state in which the iron attracted the needle the contrary way to what it did when it was cold, viz. if the bar and compass were so situated that the *north* end of the needle was drawn towards it when cold, the *south* end was attracted during the interval above alluded to, or while the iron was passing through the shades of colours, denoted by the workman the bright-red and red heat."

After noticing the results hitherto obtained relative to the magnetic action of heated iron, and showing how the contradictory statements on the subject may be reconciled, by supposing that the observations were made with iron at different degrees of heat, Mr. Barlow proceeds to describe some preliminary experiments "on the anomalous attraction of heated iron which takes place while the metal retains the bright-red and red heat;" and he then gives a table containing the results of a regular series of experiments on the subject, amounting in number to 38. These experiments were all made with bars of cast and of malleable iron inclined in the direction of the dipping needle, and, what is somewhat unappropriately called, *the negative attraction* was found to be the greatest where the natural attraction was the least; that is, opposite the middle of the bar, or in the plane of no attraction. With the bar inclined at right angles to its former position, the results were not so strongly marked as in the experiments just mentioned.

Mr. Barlow shows from experiment, that these singular effects on the compass needle were not caused by the heat itself, independently of the iron, and modestly terminates his communication with the following remarks:

"The only probable explanation which I can offer, by way of accounting for these anomalies is, that the iron cooling faster towards its extremities than towards its centre, a part of the bar will become magnetic before the other part, and thereby cause a different species of attraction; but I must acknowledge that this will not satisfactorily explain all the observed phenomena. The results, however, are stated precisely as they were noted down during the experiments, and others more competent than myself will probably be able to deduce the theory of them."

XV. *Observations for ascertaining the Length of the Pendulum at Madras in the East Indies, Lat.* 13° 4′ 9·1″ *N, with the Conclusions drawn from the Same.* By John Goldingham, Esq. FRS.

The pendulum and accompanying apparatus, with which these observations were made, were precisely the same, in all their parts, as those used by Capt. Kater at the different stations in the trigonometrical survey of England, and which have been described in the Philosophical Transactions for 1819. The results obtained with them were as follows:

By the first series of observations, the length of the seconds pendulum at Madras was 39·026323087 inches; by the second series, 39·026280447 inches.

"The mean of both is 39·026302 inches, being, according to Sir George Shuckburgh's scale, the length of the seconds pendulum by these experiments at Madras in lat. 13° 4′ 9·1″ N. at the level of the sea, in vacuo, and at a temperature of 70° of Fahrenheit."

"Then comparing this length with 39·142213 inches, the length in latitude 51° 31′ 8·4″ N. as before stated, the diminution of gravity from the pole to the equator will be ·0052894, and the ellipticity $\frac{1}{297\cdot56}$ nearly."

XVI. *Account of an Assemblage of Fossil Teeth and Bones discovered in a Cave at Kirkdale, in Yorkshire.* By the Rev. W. Buckland, Professor of Geology in the University of Oxford, &c. &c.

This highly interesting paper has already appeared in the *Annals.*

XVII. *Communication of a curious Appearance lately observed upon the Moon.* By the Rev. Fearon Fallows. (In a Letter addressed to John Barrow, Esq. FRS.)

Mr. Fallows, who is the astronomer at the new observatory founded at the Cape of Good Hope, observed on Nov. 28, 1821, a whitish spot on the dark part of the moon's limb, sufficiently luminous to be seen with the naked eye, and which now and then seemed to flash with considerable lustre. When examined with an achromatic telescope, four feet long, and magnifying 100 times, it seemed like a star of the sixth magnitude, with three other spots much smaller, one of which was more brilliant than that first noticed. The largest was surrounded by a nebulous appearance, which could not be perceived about the smallest, and the two others were similar to faint nebulæ, increasing in intensity towards the middle, but without any defined luminous point. On the 29th, the large spot was as bright as before, two others were nearly invisible, and the small brilliant one had disappeared.

XVIII. *On the Difference in the Appearance of the Teeth and the Shape of the Skull in different Species of Seals.* By Sir Everard Home, Bart. VPRS.

This notice is accompanied by three plates, showing the great difference existing between the skulls and teeth of three seals; one from the South Seas, another from the Orkney Isles, and a third from New Georgia. Sir Everard conceives that the knowledge they impart will be an advantage, when fossil remains of the seal shall be met with.

The mean height of Six's thermometer, in the year 1821, is stated in the Meteorological Journal kept at the Society's apartments, to have been 51·8°; the mean height of the barometer 29·86 in.; and the quantity of rain for the year 23·567 inches.

2. *A Letter to Sir Humphry Davy, Bart. President of the Royal Society, &c. &c. on the Application of Machinery to the Purpose of Calculating and Printing Mathematical Tables,* From Charles Babbage, Esq. MA. FRS. L. & E. Member of the Cambridge Philosophical Society, Secretary of the Astronomical Society of London, and Correspondent of the Philomathic Society of Paris. London, 1822.

We had occasion, a few months since, to notice a work, in which is detailed the progressive improvement and present high state of perfection and importance of the Steam-Engine, a machine which has been productive of such stupendous effects in its application to the arts and manufactures. We have now to state the applications of an invention in mechanics, which is calculated, extraordinary as it may appear, to produce as momentous consequences in science, by the substitution of its movements for intellectual labour, as those to which the steam-engine has given rise, in the arts of civilized society, by the abridgment of bodily toil.

The high rank which Mr. Babbage sustains as a mathematician must be well known to our readers. He commences the letter before us by stating that the great interest which has been taken by the distinguished character to whom it is addressed, in the success of the system of contrivances to which it relates, has induced him to adopt this mode of making known the principles and probable consequences of those contrivances. He observes, that the fatiguing labour and monotony of a continued repetition of similar arithmetical calculations, first excited the desire, and then suggested the idea, of a machine, which should become a substitute for one of the lowest operations of human intellect: he then proceeds as follows:

"The first engine of which drawings were made was one which is capable of computing any table by the aid of differences, whether they are positive or negative, or of both kinds. With respect to the number of the order of differences, the nature of the machinery did not in my own opinion, nor in that of a skilful mechanic whom I consulted, appear to be restricted to any very limited number; and I should venture to construct one with ten or a dozen orders with perfect confidence. One remarkable property of this machine is, that the greater the number of differences, the more the engine will outstrip the most rapid calculator.

"By the application of certain parts of no great degree of complexity, this may be converted into a machine for extracting the roots of equations, and consequently the roots of numbers: and the extent of the approximation depends on the magnitude of the machine.

"Of a machine for multiplying any number of figures (m) by any other number (n), I have several sketches; but it is not yet brought to that degree of perfection which I should wish to give it before it is to be executed.

"I have also certain principles by which, if it should be desirable, a table of prime numbers might be made, extending from 0 to ten millions.

"Another machine, whose plans are much more advanced than several of those just named, is one for constructing tables which have no order of differences constant.

"A vast variety of equations of finite differences may by its means be solved, and a variety of tables, which could be produced in successive parts by the first machine I have mentioned, could be calculated by the latter one with a still less exertion of human thought. Another and very remarkable point in the structure of this machine is, that it will calculate tables governed by laws which have not been hitherto shown to be explicitly determinable, or that it will solve equations for which analytical methods of solution have not yet been contrived.

"Supposing these engines executed, there would yet be wanting other means to ensure the accuracy of the printed tables to be produced by them.

"The errors of the persons employed to copy the figures presented by the engines would first interfere with their correctness. To remedy this evil, I have contrived means by which the machines themselves shall take from several boxes containing type, the numbers which they calculate, and place them side by side; thus becoming at the same time a substitute for the compositor and the computer: by which means all error in copying, as well as in printing, is removed.

"There are, however, two sources of error which have not yet been guarded against. The ten boxes with which the engine is provided contain each about 3000 types; any box having of course only those of one number in it. It may happen that the person employed in filling these boxes shall accidentally place a wrong type in some of them; as for instance, the number 2 in the boxes which ought only to contain 7s. When these boxes are delivered to the superintendant of the engine, I have provided a simple and effectual means by which he shall in less than half an hour ascertain whether, among these 30,000 types, there be any individual misplaced or even inverted. The other cause of error to which I have alluded, arises from the type falling out when the page has been set up: this I have rendered impossible by means of a similar kind."

The inventor of these wonderful contrivances next adverts to the quantity of typographical errors, in tables even of the greatest credit; and then after remarking that to bring to perfection his various machinery would require a great expense, both of time and money, he describes the progress which he has made in these terms:

"Of the greater part of that which has been mentioned, I have at present contented myself with sketches on paper, accompanied by short memorandums, by which I might at any time more fully develop the contrivances; and where any new principles are introduced I have had models executed in order to examine their actions. For the purpose of demonstrating the practicability of these views, I have chosen the engine for differences, and have constructed one of them which will produce any tables whose second differences are constant. Its size is the same as that which I should propose for any more extensive one of the same kind: the chief difference would be, that in one intended for use there would be a greater repetition of the same parts in order to adapt it to the calculation of a larger number of figures. Of the action of this engine, you have yourself had opportunities of judging, and I will only at present mention a few trials which have since been made by some scientific gentlemen to whom it has been shown, in order to determine the rapidity with which it calculates. The computed table is presented to the eye at two opposite sides of the machine; and a friend having undertaken to write down the numbers as they appeared, it proceeded to make a table from the formula $x^2 + x + 41$. In the earlier numbers my friend, in writing quickly, rather more than kept pace with the engine; but as soon as four figures were required, the machine was at least equal in speed to the writer.

"In another trial it was found that 30 numbers of the same table were calculated in two minutes and thirty seconds: as these contained 82 figures, the engine produced 33 every minute.

"In another trial it produced figures at the rate of 44 in a minute. As the machine may be made to move uniformly by a weight, this rate might be maintained for any length of time, and I believe few writers would be found to copy with equal speed for many hours together. Imperfect as a first machine generally is, and suffering as this particular one does from great defect in the workmanship, I have every reason to be satisfied with the accuracy of its computations; and by the few skilful mechanics to whom I have in confidence shown it, I am assured that its principles are such that it may be carried to any extent. In fact, the parts of which it consists are few but frequently repeated, resembling in this respect the arithmetic to which it is applied, which, by the aid of a few digits often repeated, produces all the wide variety of number. The wheels of which it consists are numerous, but few move at the same time; and I have employed a principle by which any small error that may arise from accident or bad workmanship is corrected as soon as it is produced, in such a manner as effectually to prevent any accumulation of small errors from producing a wrong figure in the calculation.

"Of those contrivances by which the composition is to be effected, I have made many experiments and several models; the results of these leave me no reason to doubt of success, which is still further confirmed by a working model that is just finished."

A method of determining the existence of error, should it by possibility arise in any page that the engine has composed, is also mentioned; with a few of the variety of tables which could be calculated by that actually constructed. "The tables of powers and products published at the expense of the Board of Longitude, and calculated by Dr. Hutton, were solely executed by the method of differences; and other tables of the roots of numbers have been calculated by the same gentlemen on similar principles."

In order to show the mental labour that may be saved by the employment of his machine, Mr. Babbage describes the course which was pursued, in the formation of the tables computed under the direction of M. Prony by order of the French government, constituting one of the most stupendous monuments of arithmetical calculation which the world has yet produced, and occupying 17 large folio volumes. This enormous mass of computation, one table of which must contain about eight millions of figures, was executed by three sections of calculators. The first of them comprised five or six mathematicians of the highest merit, who investigated and determined on the formulæ to be employed. The second consisted of seven or eight skilful calculators, habituated both to analytical and arithmetical computations; these received the formulæ from the first section, converted them into numbers, and furnished the proper differences to the third, which was constituted of from 60 to 80 persons. The most laborious part of the operations devolved upon the latter, few of whom were acquainted with more than the first rules of arithmetic; they rendered the calculated results in two independent sets, to the second section, for the purpose of verification.

Now it appears that if these or any other tables of similar extent, were to be computed by the aid of Mr. Babbage's engine, the number of calculators would be diminished from 96 to 12, or even less; so that the tables could be produced at a much cheaper rate, and of superior accuracy.

Another class of tables of the greatest importance is noticed, almost the whole of which are capable of being calculated by the method of differences; this includes all astronomical tables for determining the positions of the sun or planets. Mr. Babbage terminates his important communication with the following judicious remarks:

"I am aware that the statements contained in this letter may perhaps be viewed as something more than Utopian, and that the philosophers of Laputa may be called up to dispute my claim to originality. Should such be the case, I hope the resemblance will be found to adhere to the nature of the subject rather than to the manner in which it has been treated. Conscious, from my own experience, of the difficulty of convincing those who are but little skilled in mathematical knowledge, of the possibility of making a machine which shall perform calculations, I was naturally anxious, in introducing it to the public, to appeal to the testimony of one so distinguished in the records of British science. Of the extent to which the machinery whose nature I have described may be carried, opinions will necessarily fluctuate, until experiment shall have finally decided their relative value; but of that engine which already exists I think I shall be supported, both by yourself and by several scientific friends who have examined it, in stating that it performs with rapidity and precision all those calculations for which it was designed.

"Whether I shall construct a larger engine of this kind, and bring to perfection the others I have described, will in a great measure depend on the nature of the encouragement I may receive.

"Induced, by a conviction of the great utility of such engines, to withdraw for some time my attention from a subject on which it has been engaged during several years, and which possesses charms of a higher order, I have now arrived at a point where success is no longer doubtful. It must, however, be attained at a very considerable expense, which would not probably be replaced, by the works it might produce, for a long period of time, and which is an undertaking I should feel unwilling to commence, as altogether foreign to my habits and pursuits."

Article XIII.

Proceedings of Philosophical Societies.

GEOLOGICAL SOCIETY.

June 21.—Memorandum on a Substance contained in the Interior of certain Chalk Flints, by the Rev. J. J. Conybeare, MGS.

The substance in question is a white powder, giving on a rude analysis, carbonate of lime (slightly tinged by iron), 72; silex (in the state of a fine sand), 28; = 100. The nodule of flint which contained it, presented no apparent trace of any aperture by which it could have entered.

Memorandum on the Comparative Fusibility of certain Rocks, and the Character of the Results, by the Rev. J. J. Conybeare, MGS.

These experiments were undertaken chiefly with a view of comparing the characters of the indurated lias shale (found in contact with the whin dykes) of the north of Ireland with those of certain rocks to which it has been supposed to bear an analogy. The results tend (in the opinion of the writer) to establish the identity of the Irish rock with the shale of the lias formation, as occurring elsewhere, rather than with the true flinty slate or any other variety of basalt. Some experiments of the same nature on other rocks and artificial mixtures of mineral substances are subjoined. They scarcely admit of an abstract.

Article XIV.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Effect of Fixed Oils in destroying the Smell of essential Oils.*

Mr. Davies, druggist, of Chester, lately pointed out to a friend of the Editor, a fact which had accidentally occurred to him, and which he had not seen noticed in chemical books. A mixture of equal parts *of castor-oil* and peppermint water gradually loses the taste and smell of peppermint, and in a day or two is entirely deprived of it. The same effect is produced on other distilled waters, and on mixtures of other essential oils with water in the proportion of one drop to two ounce measures of water. When, however, olive oil is substituted for castor oil, a nice palate may distinguish a very slight flavour.

II. *Analyses of Magnesite.* By M. Berthier.*

This substance, commonly denominated *meerschaum*, forms with water a viscous and slightly plastic paste, resembling that of starch; it is easily attacked by the strong acids, and it gelatinizes with them: it contains much water of combination, which it gives up entirely at a red heat, without changing its form, or losing its consistence. It consists essentially of silicate of magnesia; but this is almost always mixed with some clay or silicate of alumina.

The analyses were effected in the following manner: The quantity of water was estimated from the loss of weight by calcination; a portion uncalcined was treated with nitric acid, mixed with a little muriatic acid, the solution evaporated to dryness, and treated with acidulated water. The residue was gelatinous silica, mixed, perhaps, with quartz and clay not acted upon by the acids. This, after being calcined, was boiled with liquid potash, which dissolved all the silica; and the undissolved portion was examined by the usual means. The nitric solution was deprived of its alumina and iron by hydrosulphuret of potash, and of its magnesia by potash; the amount of the latter being estimated par difference."

	Magnesite from				
	Asia Minor.	Cabanas, near Madrid.	Coulommiers.	Salinelle dep. du Gard.	Saint-Ouen au pied de Montmartre.
Silica	0·500	0·538	0·540	0·510	0·510
Magnesia	0·250	0·238	0·240	0·198	0·134
Water	0·250	0·200	0·200	0·220	0·182
Alumina	—	0·012	0·014	0·044	0·170
Oxide of iron	—	—			
Sand	—	—	—	0·028	—
	1·000	0·988	0·994	·1000	0·996

It is evident, says M. Berthier, that these five varieties belong to the same species, and that this species is essentially composed of silica, magnesia, and water; but it is very difficult to obtain a certain knowledge of the relative proportions of these principles, because there are no means of ascertaining the quantity of silica which is found in combination with the alumina.

He concludes, however, after some theoretical reasoning, that the following formula represents the composition of magnesite. $6\ M\ S^3\ Aq^2 + M\ Aq^2$.—(Annales des Mines, vii. 313.)

III. *Analyses of Native Carbonate of Magnesia.* By the Same.

Carbonate of magnesia is found, either in combination or in mixture, in a great number of limestones; but it also exists in other associations. At Baldissero and at Castella-Monte, it is mixed with silicate of magnesia and with quartz; and in the Isle of Elba, it is mixed with pure silica, which is in a particular state. Four varieties of this carbonate were subjected to an analytical process similar to that described in the foregoing article; the results were as follows:

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* The subjects of this notice, in conjunction with those of the following one, constitute the fourth section of a paper by M. Brongniart on the magnesite of the basin of Paris, &c.: an abstract of the preceding sections will appear in our next.

	Baldissero.	Castella-Monte.	Elba, No. 1. (Campo).	Elba, No. 2.
Magnesia	0·440	0·255	0·350	0·230
Carbonic acid	0·418	0·105	0·374	0·360
Silica	0·094	0·435	0·266	0·206
Water	0·048	0·120	0·010	0·045
Lime	—	—	—	0·140
Quartz	—	0·085	—	—
	1·000	1·000	1·000	0·981
Or				
Carbonate of mag.	0·810	0·203	0·724	0·480
Carbonate of lime	—	—	—	0·250
Silica	0·094	0·435	0·266	0·206
Magnesia	0·050	0·157	—	—

The quantity of water was obtained by distilling it from the mineral in a retort into a small glass tube; that of carbonic acid by calcination, the weight of the water being subtracted from the loss.

In the mineral of Baldissero, the magnesia which is not combined with carbonic acid, bears the same proportion to the silica as it does in magnesite. In that of Castella-Monte, the proportion of the latter is much greater; and in those of Elba, the magnesia is entirely saturated, leaving the silica free: this silica, however, like that in the calcareous deposits of certain mineral waters, is as readily soluble in alkaline solutions as if it had been obtained in the decomposition of a silicate by an acid.

When the carbonate of Campo is treated with a strong acid in a boiling state, the magnesia is gradually dissolved with effervescence; but the fragment neither changes its form, nor wholly loses its cohesion; when the solution is poured off, it is found to be semi-transparent like hydrophane, but by desiccation it becomes opaque, and of a very beautiful white. It dissolves without residue in boiling liquid potash, and the solution gelatinizes with acids.

"It results from these experiments, that it is not always possible to isolate by means of alkaline solutions, native uncombined silica, from the silica which we separate from a combination by an acid."—(Ibid, p. 315.)

IV. *On the Greek Fire of the Middle Ages.* By Dr. Mac Culloch.

In No. xxvii. of the Journal of the Royal Institution, is an interesting memoir by Dr. Macculloch, respecting the history and nature of this celebrated subject of inquiry and discussion. The following is a condensed view of the investigation.

The subject of the Greek fire, sufficiently obscure in itself, appears to have been rendered much more so, by collateral causes, and principally by that love of the marvellous in which mankind love to indulge. The historians who have related its effects, and of whom some have even pretended to describe its composition, have involved the subject in perplexities very difficult to disentangle; while succeeding antiquaries and historians, their analysts, have had little better success.

Dr. Mac Culloch apprehends that different inventions, and different kinds of Greek fire, have been described by the same name; that the main source of the confusion can be traced to this cause; and that there is an intimate connexion between the history of the Greek fire, and that of gunpowder.

The common opinion is, that the Greek fire was invented during the reign of Constantine Pogonatus, in the year 668, by Callinicus, an architect of Heliopolis; it was confined, according to Gibbon, for 400 years to the eastern Romans; he adds, that at the end of the eleventh century, the Pisans suffered from it without knowing its composition, and concludes with saying, that it was at length discovered or stolen by the Mahometans; and that in the holy wars of Syria and Egypt, they retorted an invention, contrived against themselves, on the heads of the Christians.

Dr. Mac Culloch observes, respecting this statement, that "the communication between Heliopolis and the eastern nations, renders it, in the first place, suspicious, that the Greek architect borrowed the invention from the orientals. That they possessed it at least before the Greeks, whether they communicated it or not, appears to me as capable of proof as can be expected under similar circumstances. When Gibbon says, that the Mahometans borrowed the invention from the Christians during the wars of the crusades, he forgets that the Arabians learned their chemistry from the Egyptians, by whom that art was practised 300 years at least before the time of Mahomet. That they also borrowed from a still more distant oriental source, appears equally certain."

Naphtha is said to have been one of the chief ingredients in this composition; and that substance is well known to be very common in many parts of the ancient Persian kingdom and in India. Now it is much more probable, that a burning compound in which naphtha was an ingredient, should have been invented where that substance abounded, than where it was unknown; and if it can be proved that the use of inflammable compositions was known to the eastern nations before the time of Callinicus, his claim to this invention falls to the ground. It might, however, have spread among the later Arabians from the Greeks; it became common, and probably from this very source, in the wars of the crusades; "but it is also possible that this, or one of the different inventions known by the same name, might have been discovered by the Arabians themselves, who were then much addicted to chemical pursuits."

One at least of the Greek fires of the crusades was a composition into which nitre entered, and, therefore, depending on the same principle as gunpowder; and thus the two inventions are connected. The art of making fire-works appears to be the original invention, and to have been the true parent of gunpowder, ancient as well as modern. There seems abundant reason to suppose that the cradle of pyrotechny was in the east; in China, the use of fireworks for amusement has been known from a period beyond all record; and in India, the use of rockets for military purposes is of an antiquity equally obscure.

After some observations on the close analogy which all pyrotechnical compositions bear to gunpowder, Dr. Mac Culloch attempts to trace backwards to the oldest records extant, respecting any preparations of this nature; and these lead us to India, as before observed.

In Grey's Gunnery, printed in London in 1731, is a passage deduced by Philostratus from the life of Apollonius Tyanæus; in this it is said, that Alexander the Great never entered the country of "the truly wise men who dwell between the Hyphasis and the Ganges," "deterred, not by fear of the inhabitants, but as I suppose, by religious considerations," "for these holy men, beloved by the gods, overthrow their enemies with tempests, and thunderbolts shot from their walls." The Egyptian Hercules and Bacchus are likewise said to have been repulsed from the cities of these people, who were the Oxydracæ, by lightning and thunderbolts hurled on them from above. Gunpowder is mentioned in the code of Hindoo laws, which is supposed to reach back to the time of Moses; and these testimonies are confirmed by a passage in Quintus Curtius, mentioning a compound possessed of similar qualities. Dr. Mac Culloch thinks, however, that the story of the Oxydracæ alludes to some kind of rocket.

"If thus far is right, the claims of the early orientals to the Greek fire is established. The Greeks might have received it from the Arabians, or from a more direct source; but it seems likely that Western Europe, at least, is indebted to this people for its knowledge of pyrotechny." It is then shown that this art is of more ancient date among us than is commonly imagined; and having, as above, traced generally the origin of pyrotechny from the east, Dr. M. proceeds to see if some of the particular inflammable compounds, known by the name of the Greek fire, cannot be traced thither also. It is reported by the author of the *Esprit des Croissades*, to have been known in China in the year 917, and as the Chinese have never been known to borrow arts from the Europeans, and were acquainted with the properly explosive compounds, it is most likely that it was known to them long before. It is said to have been known in China by the name of the oil of the cruel fire. Thus the oily or resinous Greek fire seems to claim an oriental origin as well as the explosive and combustible nitrous compounds.

The Byzantine writers are our earliest European authorities for the names, composition, and effects, of the Greek fire. The Greeks called it the liquid, or maritime fire, probably from its application in naval engagements; "Procopius, in his history of the Goths, uses the same term as the Chinese, calling it an oil, Media's oil, as if it had been some infernal composition of that noted sorceress. But the historian seems to have borrowed this term from Pliny, who calls naphtha *ἐλαιον Μηδικαξ*, a sort of proof, by the way, that naphtha entered its composition." * Cinnamus also calls it *πυρ Μηδικον*; and all these names bespeak some resinous or oily inflammable compound, such as might be used without the help of nitre. But from the name given to it by Leo, we must conclude that he is speaking of some explosive substance, into which nitre entered as an ingredient.

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* There is a little confusion in this passage of Dr. Mac Culloch's valuable memoir, which appears to have arisen in part from a somewhat obscure note in Gibbon's History, quarto edition, vol. v. p. 402. Procopius, in his account of the celebrated siege of Petra, describes the use of what must have been a variety of the Greek fire, and says *that it consisted* of sulphur, and of bitumen which the Medes called naphtha, and the

"All the descriptions of the composition of the Greek fire seem to refer to resinous and oily substances; by some writers, it is said to have been unctuous and viscid; while others again describe it as a solid substance. "Quintus Curtius considers it as made of turpentine. Anna Comnena says, that it was composed of sulphur, bitumen, and naphtha. In another place she says, that it was a mixture of pitch and other similar resins, and that it was thrown from balistæ, and attached to arrows."

From the various modes in which it is said to have been used, it appears that at least two kinds of military fireworks are described under a common name; one of these may have been a merely inflammable resinous composition, while it is likely that the other was a nitrous compound projected from balistæ in some kind of carcasses. From an account in a French Chronicle of 1190, it would appear that it was a liquid, inclosed in vessels of some kind, "phioles." "This was then that liquid fire that is said to have been used by hand at sea, or in close action, and which is also said to have been thrown by means of military engines in sieges. It is evident that this is not Anna Comnena's fire, for it could not well be thrown from balistæ, or attached to arrows; unless we imagine that it was always used with tow as before mentioned, [tow being dipped in it, and wrapped round arrows.] Hers appears rather to have been a solid composition. It disagrees still more with that of Leo and Joinville."

It being impossible to reconcile this description to any imaginable composition or effects, Dr. Mac Culloch gives up the point as unintelligible; and observes, "We cannot suppose the liquid in the 'phioles' to have contained nitre, because that salt will not mix with any liquid of this nature in such a manner as to aid its combustion."

"The descriptions which represent the Greek fire as unctuous and viscid, and as adhering to the objects which it reached, may be, perhaps, reconciled to the former, since a viscid substance, as well as a liquid one, might have been kept in 'phioles.' They might easily have been all formed of the same resinous ingredients in various proportions."

"The opinion of the Greek fire being inextinguishable by water could not justly have been entertained of any compositions of this nature, not even of Anna Comnena's sulphureous compound. No burning substance could have resisted an application of this nature, provided it were employed in sufficient quantity, unless under the protection of a carcass or tube of some kind, in which case it must also have contained nitre."

"That sand should have extinguished some of these fires, as related by the Florentine monk who describes the siege of Acre, we can under-

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Greeks, ἔλαιον Μηδείας, or the oil of Medea. It is Pliny alone who ostensibly refers to the sorceress, and he does not allude to the Greek fire; and of course does not give naphtha a Greek name. As we know that the Medes used in war arrows smeared with naphtha, and inflamed, it seems probable that the appellation cited by Procopius, notwithstanding its orthography, refers merely to the country, and not to the enchantress.—(*Procop. de Bell. Gothic.* l. iv. c. 11. Plin. Hist. Nat. ii. 109.)

stand; but that it should have been put out by vinegar and urine, and not by water, as he also affirms, is impossible, as these were not likely to have been procured in sufficient quantity, surely not in such abundance as water; and on no other principle could the one have acted better than the other."

"I do not see," continues Dr. M. "that any further light can be thrown on these varieties of the Greek fire. The accounts seem to be confused and unintelligible, (as far as they are so,) partly by the ignorance, and partly by the exaggeration, of the reporters. Abstracting these, it is probable that they were truly enough, as has been said, resinous inflammable compounds, solid, tenacious, or liquid, without nitre, and exactly similar to the fires of our ancient fire ships, before chemistry had taught us to proceed on better principles."

"Joinville's description will be found much more intelligible, and will, I think, fully prove the supposition that there were different things known by one name, and that the Greek fire used against Louis at Acre was neither the Chinese oil, nor any viscid substance, nor even the composition described by our celebrated female historian."

According to Joinville, the Greek fire was thrown from the walls of Acre by a machine, called a petrary, three times, and from a cross-bow four times, in the course of the night. It is described as coming forward "as large as a barrel of verjuice, with a tail issuing from it as big as a great sword; making a noise in its passage like thunder, and seeming like a dragon flying through the air; while, from the great quantity of fire which it threw out, it gave such a light that one might see in the camp as if it had been day."

After an examination of this account, Dr. Mac Culloch concludes, that this was a firework of the rocket kind, "without a bore, and therefore incapable of flying by its own recoil; in short, a huge squib. Such a firework as this would produce all the appearances described; the long tail of fire, the noise, and the light; and it would require a projectile force, which might have been given both by mechanical and chemical artillery, by the balista, and by the petrary or mortar."

"If I am thus right," he continues, "in supposing the Greek fire of Joinville to have been a rocket of this imperfect kind, it is easy to explain the resistance which it offered to any attempts to extinguish it. Water has no effect, because the blast from the surface prevents it from entering; for the vinegar and urine, the good monk must be held responsible. It is pretty clear that his account of this property in the Greek fire has been derived from these very fireworks, and has, by the usual mistake, been assigned to the whole race."

"As no further light can be thrown on this subject from the ancient authors, it is unnecessary to prolong this inquiry. The subject seems to be cleared, at least, of much of its mystery; and that this mystery has in great measure arisen from mistakes and exaggerations, must be very apparent. We may remain at our ease on this head, and be satisfied that we have lost nothing by our imaginary loss of the Greek fire. We may still safely boast, that in whatever arts either the Greeks or Arabs may have excelled us, in that of destroying each other we could have taught them much, and could have learned *nothing from them.* Divested of the mist which wonder and ignorance have drawn round it, the boasted Greek fire seems to have been a contemptible weapon enough. Had the rhyming monk of St. Louis been at the sieges of Copenhagen or Algiers, it would be difficult to conjecture where they would have found words to express what must have been, to their fires, like the thunders and lightnings of heaven to those of the theatre."

V. *Royal Institution of Cornwall.*

The Report of the Council to the Fourth Annual Meeting of this Institution, the establishment of which, before it received the honour of Royal patronage, is noticed in the old series of the *Annals*, vol. xiv. p. 395, presents some gratifying indications of the progress of science and literature in the county of Cornwall, the mineral structure and riches of which offer so many subjects for philosophical investigation.

The Institution possesses a select library, a zoological collection, many objects of antiquarian research, an elegant apparatus for experiments, and an increasing collection of minerals. The Council entertain hopes that the time is not far distant when an exhibition of paintings will also be established; they observe, that natives who have made no small proficiency in the art of painting are to be found in towns, in the village, in the hamlet; and that the cherishing beam of the public eye is only wanted to bring them into notice.

A seal has been made for the Society; and the first diploma under it, constitutes Sir Humphry Davy an honorary member, to which distinction he was elected by a special general meeting convened for the purpose; the Society hoping, that by showing their regard for distinguished characters in science, literature, and the arts, they are using their endeavours to strengthen those ties by which all liberal pursuits are connected.

VI. *Alkohometrical Application of the Thermometer.*

M. F. Groening, of Copenhagen, has discovered that the thermometer may be successfully used in distillation, as an alkohometer. He observed, while comparing the temperature of the interior of the rectifier with that of the water about it, in a distilling apparatus invented by himself, that the thermometer always rose to a certain point, for example 65° Reaumur, or 179° Fahrenheit, before the first drop of the distilled liquor appeared; and, likewise, that it remained at that point till about half the fluid in the retort was evaporated, but then, by degrees, at first slowly, afterwards more rapidly, rose to 80° Reaumur, or 212° Fahr.

By trials with the alkohometer, he found that as long as the thermometer remained at a certain point, the liquor which came over was of an uniform strength, but when it rose the liquor grew weaker and weaker, till at last mere water came over, namely, when the instrument had attained the height of 80° Reaumur.

The results of M. Groening's experiments, which were performed many times, and which of course depend on the different temperatures of the vapours of alcohol and water, were as follows:

1. A person may, by the state of the thermometer, immediately ascertain the strength of the liquor in the vessel.

2. There is no necessity of using the alkohometer in distillation, as the thermometer indicates the strength of the liquor with equal accuracy.

3. Without drawing off any spirit, what quantity there is of any particular strength may be immediately known.

4. Every possible fraud, during the operation, may be prevented, as the apparatus can either be locked up or brought into an adjoining apartment, for the person who attends the work does not require the thermometer to direct him.—(Edin. Phil. Journ. vii. p. 214.)

VII. *Tabular Spar, Colophonite, and Pyroxene.*

Mr. H. Seybert, of Philadelphia, has analyzed the above minerals from the vicinity of Willsborough, Lake Champlain. He found the tabular spar to contain

Silica	51·00
Lime	46·00
Alumina and oxide of iron	1·33
Water	1·00
Magnesia and loss	0·67
	100·00

This statement agrees very nearly with M. Bonsdorff's analysis of the same mineral from Pargas (*Annals*, Oct. 1820). Mr. Bonsdorff obtained

Silica	52·58
Lime	44·45
Magnesia	0·68
Protoxide of iron	1·13
Volatile matter	0·99
A trace of alumina and loss	0·17
	100·00

Mr. Seybert observes that this mineral is a bisilicate of lime, which, adopting Dr. Thomson's numbers for silica and lime, appears to be the case. If it consisted precisely of two atoms of silica and one of lime, the proportions would be 51 silica, and 44·62 lime, which agree still more nearly with M. Bonsdorff's analysis.

Mr. Seybert observes, that "it is an interesting fact, that this mineral, whether found in Hungary, Sweden, or in the United States, is constantly associated with substances of corresponding characters; that of Dognarka is united with brown crystallized garnets and blue calcareous spar; that of Pargas with black sphene, an amorphous mineral, of a reddish colour, resembling idocrase or garnet, and small grains of a green substance, resembling actynolite, (probably pyroxene); that of the United States, with colophonite and pyroxene.

The colophonite yielded

Silica	38·00
Lime	29·00
Protoxide of iron	25·20
Alumina	6·00
Water	0·33
	98·53
Loss	1·47
	100·00

The pyroxene yielded

Silica	50·33
Protoxide of iron	20·40
Lime	19·33
Magnesia	6·83
Alumina	1·53
Water	0·66
	99·08
A trace of oxide of manganese and loss	0·92
	100·00

(Silliman's Journal.)

VIII. *Death of Dr. Marcet.*

We lament to state the demise of Dr. Marcet, that took place on Saturday, Oct. 19. His chemical researches were chiefly detailed in the Transactions of the Royal Society, of which he was an active member. His principal work is a treatise on Calculi, a book of established reputation, and displaying the minute accuracy with which all that he performed is replete. He was in the 52d year of his age, and was about to return to Geneva, his native country.

IX. *Death of Mr. James Sowerby.*

It is with great regret also that we have to announce the death of this gentleman which occurred on Oct. 25, after a long and severe illness. Mr. Sowerby was a Fellow of the Linnæan Society of London, Member of the Geological Society, Honorary Member of the Physical Society of Gottingen, &c. &c. His patient and indefatigable labours in several branches of natural history are well known to the scientific world; and he contributed in various ways to the advancement of natural knowledge.

ARTICLE XV.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

The Life and Remains of the late Dr. Edward Daniel Clarke, Professor of Mineralogy in the University of Cambridge.

A Quarto Volume, with Engravings, will shortly appear, giving an Account of Don Antonio del Rio's Discovery of an ancient City in the Kingdom of Guatimala, North America.

JUST PUBLISHED.

A Treatise on the Foot-rot in Sheep, including Remarks on the exciting Cause, Method of Cure, and Means of preventing that destructive Malady; being the Subject of three Lectures delivered in the Theatre of the Dublin Royal Society. By Thomas Peall, Esq. Veterinary Professor to that Society.

A Practical Treatise on Diseases of the Heart. By Henry Reader, MD. Physician to the South London Dispensary, &c.

M. C. Pfeiffer, of Cassel, has lately produced a beautiful Work on the Land and Fresh-water Mollusca of Germany. 4to. With Eight Plates. The work is in German, but the specific characters are given in Latin.

A Treatise on the Utility of Sangui-suction, or Leech-bleeding. By Rees Price, MD. 12mo. *3s. 6d.*

Researches respecting the Medical Powers of Chlorine, particularly in Diseases of the Liver; with an Account of a new Method of applying this Agent, by which its Influence on the System can be secured. By William Wallace, MD. MRIA. MRCS. Ireland, &c. 8vo. 6s.

ARTICLE XVI.

NEW PATENTS.

Sir A. Perrier, City of Cork, Knt.; for improvements in the apparatus for distilling, boiling, and concentrating, by evaporation, various sorts of liquids.—July 27.

R. B. Roxby, Arbour-street, Stepney, Gent. for certain improvements on the quadrant.—July 31.

W. Cleland, Glasgow, Gent. for an improved apparatus for evaporating liquids.—Aug. 17.

D. Mushet, Coleford, Gloucestershire, iron-maker, for an improvement or improvements in the making or manufacturing of iron from certain slags or cinders produced in the working or making of that metal.—Aug. 20.

W. Mitchell, Glasgow, silversmith, for a process, whereby gold and silver plate, and other plate formed of ductile metals, may be manufactured in a more perfect and expeditious manner, than by any process which has hitherto been employed.—Aug. 24.

T. Sowerby, Bishopwearmouth, Durham, merchant, for a chain upon a new and improved principle, suitable for ships' cables, and other purposes.—Aug. 29.

R. Vazie, Chasewater, Mine Kenwyn, Cornwall, civil engineer, for an improvement in the compounding of different species of metals.—Sept. 3.

H. Burgess, Miles's-lane, Cannon-street, London, merchant, for improvements on wheel-carriages.—Sept. 3.

ARTICLE XVII.

METEOROLOGICAL TABLE.

1822.	Wind.	BAROMETER. Max.	BAROMETER. Min.	THERMOMETER. Max.	THERMOMETER. Min.	Evap.	Rain.	Daniell's hyg. at noon.
9th Mon.								
Sept. 1	N W	30·19	30·18	71	40	—		
2	S W	30·18	30·02	71	41	—	—	
3	N W	30·14	29·96	73	50	36		
4	N W	29·98	29·97	71	58	—		
5	S W	29·97	29·93	73	60	30	—	
6	S W	30·13	29·93	72	47	—		
7	N W	30·14	29·97	68	48	—		
8	S W	30·04	29·97	69	48	—	—	
9	W	30·22	30·04	66	45	—		
10	N W	30·25	29·98	69	42	—		
11	S W	30·13	29·86	73	46	81	—	
12	N W	30·09	30·07	64	50	—		
13	N E	30·29	30·09	64	33	—		
14	E	30·29	30·17	67	49	—		
15	E	30·17	30·15	62	43	—	05	
16	N E	30·15	30·14	70	43	—		
17	N W	30·15	30·09	77	46	—		
18	N E	30·21	30·09	70	49	—		
19	E	30·21	30·08	66	47	—		
20	S E	30·08	29·98	68	47	91		
21	N E	29·99	29·95	65	49	—		
22	E	29·99	29·94	58	52	—	27	
23	N E	29·94	29·49	44	53	—	29	
24	N E	29·49	29·45	60	50	—	85	
25	N	29·81	29·47	59	40	—		
26	N	30·22	29·81	63	43	—		
27	N	30·31	30·22	58	43	—	—	
28	N E	30·31	30·11	58	47	—		
29	N E	30·11	29·98	62	39	—		
30	N E	29·98	29·85	68	36	70		
		30·31	29·45	77	33	3·08	1·46	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that *the result is included* in the next following observation.

REMARKS.

Ninth Month.—1—7. Fine. 8. Cloudy: windy. 9, 10. Fine. 11. Cloudy: a little rain in the evening. 12. Fine. 13. Cloudy and fine. 14. Cloudy. 15. Cloudy: windy. 16. Overcast. 17—21. Fine. 22. Cloudy: windy: swallows begin to congregate. 23. Cloudy: rainy. 24. Rainy. 25. Fine. 26. Cloudy: fine. 27. Bleak. 28—30. Fine.

RESULTS.

Winds: N, 3; NE, 9; E, 4; SE, 1; SW, 5; W, 1; NW, 7.

Barometer:	Mean height	
	For the month	30·035 inches.
	For the lunar period, ending the 8th	29·990
	For 15 days, ending the 2d (moon south)	29·954
	For 13 days, ending the 15th (moon north)	30·074
	For 13 days, ending the 28th (moon south)	29·987
Thermometer:	Mean height	
	For the month	58·080
	For the lunar period	62·266
	For 31 days, the sun in Virgo	58·964
Evaporation		3·08 in.
Rain		1·48

Laboratory, Stratford, Tenth Month, [illegible] L. HOWARD.

ANNALS

OF

PHILOSOPHY.

DECEMBER, 1822.

ARTICLE I.

Sketch of the Geology of Snowdon, and the surrounding Country. By W. Phillips, FLS. MGS.; and S. Woods, MGS.

(*Concluded from* p. 335.)

From the elevated ground near Capel Curig, we could perceive in the distance a more favourable atmosphere, at the time when rain and mist prevailed on Snowdon, and the neighbouring mountains. We determined, therefore, to proceed by Llanwrst to Conway, Bangor, and Carnarvon, and to return, if possible, over Snowdon.

Between Capel Curig and Conway, we did not perceive any rock with which we were not already acquainted, the slates and lower rock of Moel Shabod prevailing every where, with the same direction of the slaty cleavage. We were particularly anxious to ascend a hill marked j in Mr. Greenough's map, and which, as well as the whole range of which it forms a part, is there represented as being crowned by transition limestone. Our anxiety was increased by having been informed by Mr. Dawson that limestone certainly is not to be found on any part of that range. We ascended the hill on the eastern side of the valley beyond Bettws, and on traversing the whole of it, we could perceive no other rock, nor even any other variety, than such as we have already noticed. As this mountain may be said to be insulated, and, therefore, not forming a part of a range (for it does not exceed about a mile and a half in length), we suspected the hill marked j in Mr. Greenough's map is part of a range still further east, apparently of a different character, being covered the whole of its considerable extent, as far as we could perceive it beyond Llanwrst, with herbage to the very summit; and as we could not detect either from the top of the hill we had ascended, or during our walk of three or four miles to Llanwrst, any lime kiln, or even any rock or important opening, on the sides of the range, we were still doubtful whether limestone formed any part of it. In answer to our inquiries, the landlord of the inn at Llanwrst, an intelligent man, who had kept it during 13 years, assured us that no limestone is found nearer to that town than 16 miles; namely, near Abergelau on the coast; thus confirming the report of Mr. Dawson, that no limestone is found on the range in question.

We have stated that on the hill we ascended, we found no variety of rock, or slate, which has not been previously described, and that the run of the cleavage is NE and SW: the dip is about 54° to the NW; and we had, as we conceived, sufficient evidence to prove that here the slates were interstratified in masses of considerable thickness, with rocks perfectly resembling those of the base of Moel Shabod; the latter forming ridges on the sides of the mountain, with alternate depressions in the spaces occupied by the slates, which, generally speaking, appear to be the most liable to decomposition; and from having a better opportunity of observing on the side of this mountain the nature of the alluvium so prevalent in many places we had visited, and which has been turned over in many instances on the side of the high road to the depth of at least 20 feet, and is of a red colour, we were persuaded that this alluvium is derived from the decomposition of the slates and other rocks every where prevalent.

While on this mountain, we were particularly struck by an appearance of stratification on the sides of another above Bettws, composed of the same rocks. This resemblance to stratification, for it is only resemblance, is in a direction not quite at right angles to the plane of the cleavage, in which there are natural fissures that in the distance seem to be perfectly parallel to each other, giving to the whole mass the aspect of regular layers or beds of the same rock. We did not view this mountain sufficiently near to discern the irregularity which we cannot doubt would be visible on the spot, because it has precisely in the distance the same character as is apparent on the northern side of Moel Shabod above the lakes of Capel Curig, and on the side above Pont y Cyffin; the latter we examined closely, and were satisfied that this resemblance to stratification is in fact only a tendency to cleavage, greatly resembling that which in the slates with which they are interstratified, often divides them into the rhombic form, but the lines of separation are not either even, or parallel with each, nor are they always continuous, but often stopped, and are again renewed above or below.

In our way from Llanwrst to Conway, we ascended about 600 feet from the road to view one of the finest waterfalls of North Wales, Rhaiadr Mawr, which is about six miles on the south of the latter place. It descends from the mountain region through a narrow chasm where the rocks are laid bare. The same rocks and slates were visible here also, the prevalent rock being that which has the most homogeneous and compact aspect, and which likewise is the prevailing rock of Moel Shabod. In some instances, it contained harder masses imbedded in it of the same composition as the rock itself, which here also contains calcareous spar and opaque felspar (?). Slates predominate just above the paper mill, situated about 100 feet above the road. From this place to Conway, the slates continue; they are often in a very decayed state, and the road, where it is repaired with them, is in many places as black as if the shale of a coal-field had been employed. The direction of the cleavage is as before noticed.

A letter from the Rev. W. D. Conybeare having mentioned the existence of a felspathic rock in the hill above Conway, with a request that it might be attentively examined, we became desirous of giving our best attention to its discovery, and were prepared to expect in the nature of the rocks we were about to observe, a great difference to those we had so lately been attending to.

This hill rises gradually at a little distance on the west of Conway, extending about three miles towards Bangor, its northern limit being the sea. We walked the greater part of the way along the summit ridge, descending to the high road, perhaps one-quarter of a mile before its termination, which is too rough and abrupt to admit of descent. In this walk, we did not perceive any well-characterised slates; the rocks, however, especially at the termination of the hill near Conway, have a decidedly slaty structure, particularly apparent on the surface, with the plane of cleavage distinctly ranging E and W; that is to say, nearly at right angles to that in which it had uniformly appeared heretofore; the dip is towards the south at a high angle.

It is often only by a very attentive comparison of a suite of rocks which in themselves are but varieties of the same composition, that we can perceive the alliance of the two extremes, which not uncommonly are so dissimilar as to appear perfectly distinct in their nature and composition. So is it with the rocks of the hill in question.

The first rock which presented itself immediately on ascending the hill, bears a general resemblance to the prevailing rock of the base of Moel Shabod in texture and aspect; it is, however, somewhat harder, and would appear perfectly homogeneous, but for the existence of a few minute specks of crystalline quartz, and also of a substance of a dark-green colour, which are assumed to be chlorite, and this continued with little variation until we had advanced much higher, when suddenly a sort of puddingstone appeared, of which the paste or basis was the same rock, and the included masses nearly in spherical nodules of a substance sometimes resembling hornstone, but mostly weathering either into hollow or concentric balls, apparently of whitish hornstone. A solid unweathered mass broken through the centre exhibits the appearance of its being formed of concentric coats, somewhat varying in colour from white to yellow; this mass does not yield to the knife. This rock prevailed for a considerable distance E and W, and about 12 to 20 feet in width, the fore mentioned rock ranging on each side of it. Suddenly it assumed a somewhat different appearance, as though it had become cellular, by the disintegration of the rock having left only the apparent hornstone, which, therefore, must have been disseminated through the mass. On this rock, we met with a range of shallow quarries, from which the vesicular stone was raised, as we have since heard, on the assumption that it might be employed for millstones in place of the French buhrstone; but the scheme was relinquished on finding that the decomposition which had produced its cavities had not proceeded far into the mass, which became softer in descending. About one quarter of a mile from these quarries, and at the edge of the cliff, there is a large and very singular mass projecting towards the sea, possessing all the external appearance of the same rock. Just beyond this, the rock first observed appeared to be traversed by veins, and to include specks and small masses of white quartz, which sometimes prevailed so greatly as to constitute a very hard rock; but in many instances, the quartz was left by the decomposition of the rock in a vesicular state, the cavities being lined with crystals of quartz, as well as all the crevices in the rock itself, the surface of the hill being strewed over for some distance with small masses or fragments of this description; and, finally, at about the place at which we descended, quartz appeared to be so finely and universally disseminated throughout the mass as to give an increased hardness to the rock, and somewhat the aspect of compact felspar, occasionally of hornstone, or of a fine-grained sandstone, and these we assume to be the varieties alluded to by the Rev. W. D. Conybeare as the felspathic rock of this hill.

The fragments scattered on the side of the remaining descent (about one-fourth of a mile), denoted the continuance of the same rock to the termination of the hill. On the side towards the high road, we saw the workings of a mine for lead, but none, as we were informed, had yet been discovered. Opposite to this felspathic-looking rock, namely, on the other side of the high road, coarse slates prevailed, interstratified with a still harder variety of rock first observed in ascending Conway Hill, but yielding to the knife, and much resembling some of the harder varieties of the rocks of Ben Glog; the direction of the cleavage was NE and SW.

Beyond the village of Duygyfylche, the road rises on the ruinous side of Penmanmaur, which is so completely covered by fallen masses of all sizes, that we cannot assert having seen a single rock *in situ*. These rocks have the aspect of a fine-grained greenstone, and consist of a greyish substance enclosing dark-green particles, having generally a crystalline aspect, but too minute to ascertain their forms even by the assistance of the glass: minute and very slender crystals of transparent felspar are occasionally observable. The rock is sometimes traversed by veins of quartz, enclosing crystals of a green substance, much resembling those of the rock itself; and we brought away some specimens containing the largest crystals we could find, in the hope of ascertaining their nature: though very minute, they cleave with brilliant surfaces, and afford by the reflective goniometer angles coinciding with those of augite. The rock is hard beneath the hammer, but is readily cut by the knife affording a grey powder; we, therefore, conclude its chief constituents to be the substance which we have termed steatite, and augite, and we are the more confirmed in this opinion from having found one variety in which the steatite extensively prevails; this yields more readily to the knife, and encloses white specks of a substance which, as it effervesces with acid, we conclude to be calcareous spar; thus evincing the connexion of this with the rocks before observed.

At the termination of Penmanmawr towards Bangor, the appearance of this rock suddenly ceases, the hills retiring further from the shore, but behind Aber, we visited an old slate quarry where the direction of the cleavage was as usual NE and SW. Between Aber and Bangor, scarcely any opportunity occurred of observing the nature of the country, the surface consisting of gentle slopes, for the most part well-covered by herbage.

The whole coast between Bangor and Conway is represented in Mr. Greenough's map as consisting of the old red sandstone; for this rock we looked carefully, but in vain. The only rock that fell under our notice that could be mistaken for the old red sandstone is that already noticed on Conway Hill, which, without sufficient attention, might be supposed to be a conglomerate.

From Bangor, we walked to Garth Ferry, and thence passed over the Menai, accompanied by Mr. Dawson and Mr. J. Woods, to the opposite ferry on the Anglesea side, in order to view the numerous trap dykes described by Mr. Henslow as occurring between the ferry-house and Beaumaris. The rock close to the ferry-house and on the shore, we assume to be Mr. Henslow's greywacke, but it has to us more the appearance of quartz rock, consisting of crystalline grains of quartz united without cement, but including here and there very minute ochreous specks, as though one of its ingredients had perished : the soundest specimens that we could obtain, however, were not without this appearance : the rock is sometimes traversed by veins of granular quartz, whiter and more compact than the rock itself.

Between the ferry-house and Lady Bulkeley's cottage, which are scarcely a mile distant, the rock is chloritic, yet often assuming the appearance of serpentine; in some places it seems to consist almost wholly of slaty chlorite and chlorite slate; in others, it consists of layers of quartz and of chlorite, or of carbonate of lime and chlorite, and occasionally it includes considerable masses of limestone. Traversing this rock, we observed seven dykes in the distance above mentioned, and reckoning from the ferry-house, the first, second, fourth, fifth, and seventh, run in the direction of NW and SE; one, the third, NE and SW; and one, the sixth, N and S. The first is about 16 feet wide; the second, 8 feet; the third, 1 foot; the fourth, fifth, and seventh, about 4 feet each; the sixth, 5 inches. These dykes do not all consist of the same variety of rock; the first, second, fourth, and seventh, are constituted chiefly of what may be termed a fine-grained basalt, often very beautiful of its kind, at once both hard and brittle. The base is a dark substance, of which it seems impossible to define the nature by its external characters, and it is rendered porphyritic by the presence of crystalline calcareous spar, very slender crystals of felspar, and small masses of iron pyrites: the whole rock, except the two latter substances, yielding pretty readily a grey powder. The third and sixth are of a much finer grain, and of a green colour almost perfectly agreeing with that of the chlorite slate which they traverse. By the assistance of the glass, they can scarcely be said to possess a granular texture, and they appear to be perfectly homogeneous without the presence of any imbedded substance; their fracture is uneven and splintery; they are so soft as to yield readily to the knife, and their aspect differs so very little from some of the steatitic rocks already described, that every one to whom they have been presented for inspection, without any information of their locality, has considered them to be merely varieties of the base rocks of Moel Shabod. A rock of the same kind is also connected with the more compact substance of the second dyke. The fifth appears to be in a state of almost thorough decomposition.

The walls of these dykes are not all equally well defined; those of the small one, the sixth, is the most completely so of the whole. In most of the others, there seems more or less an intermixture of the chloritic rock they traverse, and we did not perceive any instance in which an alteration of texture had taken place in the chlorite rock, even where, as in the instance of the second dyke, the chlorite rock protruded so far into the dyke as almost to cross it. We observed that thin veins of quartz traverse the small dyke, and the rock including it, without any alteration either of texture or direction.

At Lady Bulkeley's cottage, we ascended to the road which is from 80 to 100 feet above the sea, and walked back to the ferry-house. The first object that attracted our attention was a large mass of limestone which had been cut through in forming the road. To this succeeded chloritic rocks, of the same varieties as those below, and often including masses of limestone, but, where these were absent, exhibiting curious contortions, and in one instance the series of vandykes noticed by Mr. Henslow. We looked attentively for the continuance of the dykes, but could perceive only two of the seven, which from their situation, width, and the nature of the rock, we considered to be the first and second, consisting here and below of the same firm basaltic rock. In neither instance was the including rock altered in its direction, but in one place its texture certainly appeared to have suffered materially. Three or four feet from the dyke, it had its usual appearance of slaty chlorite of a green colour; but close to the dyke, it had no longer the appearance of a chloritic rock; it had become less firm in its texture, very brittle, and of a dingy-brown hue; and even the dyke itself at the immediate contact appeared to be less crystalline than in the interior.

Returning to Garth Ferry on the Bangor side of the Menai, we walked along the shore to Bangor Ferry, near the bridge now erecting over the Menai. For some distance, we passed only the broken edges of rocks and slates, almost perfectly resembling those of the base of Moel Shabod, to which we have been compelled so often to allude. In one instance at least, the rock had the appearance of a conglomerate, but it was acknowledged by all that the included nodules or masses were often of the same nature as the rock itself, though considerably harder, sometimes of the substance resembling hornstone occurring on the hill above Conway, the rock itself often partaking of the same nature. The plane of the slaty cleavage is here also NE and SW.

We were conducted by Mr. Dawson a little above the shore of the Menai to view a siliceous sandstone enclosing rounded masses of quartz, and having the characters of the millstone grit of Shropshire, being connected with a limestone and shale enclosing thin layers of coal. It has been described by Mr. Henslow, and apparently with much justice, as belonging to a regular coal formation, though of small extent, and without possessing any beds of workable coal. We had no opportunity of perceiving its connexion with the rocks and slates of its neighbourhood, but we visited the dyke which traverses the shale and limestone, which are quarried for building upon a large scale close to the shore. In some places the substance of the dyke is very compact, and then appears to consist chiefly of crystals of black augite enclosing carbonate of lime and iron pyrites, but is mostly in a decomposed state, and has much the appearance of a lode sand, among which may be found very small fragments of a white crystalline substance. At the base of the cliff it is of considerable width, and appears to divide into branches, but we did not perceive any alteration in the limestone traversed by it, in regard either to direction or texture; but above, where immediately in contact with the dyke, it appeared to be a little turned upward, an appearance which might be the consequence either of the unevenness of the surfaces, or of our not standing so as to bring the eye on the same plane as the stratification of the layers of the rock, the ground being very unfavourable.

As we travelled from Bangor to Carnarvon in the evening, we had no sufficient opportunity of observing the nature of the country between the two places.

As the weather had now become more favourable to the ascent of Snowdon, we determined to attempt it from the side of Llyn Cwellyn by the copper mine road, about seven miles south of Carnarvon.

The first five miles of the road from Carnarvon scarcely afforded the appearance of a rock *in situ*, but only a few boulders, the country on either side of the road being well covered by herbage, until we arrived at the foot of Moel Eilio on the left, which presented nothing near the road but slates greatly resembling some on Moel Shabod, and above Nant-francon quarries, and apparently partaking more of the nature of steatite than of common slate: they split into thin laminæ, are hard enough to scratch glass, and are green by transmitted light: they have the usual direction of NE and SW. On the right, the foot of Menydd Mawr presented a somewhat singular porphyritic rock. It appears to consist of transparent crystals of quartz and of felspar, included in a paste which, although it is hard, and yields with some difficulty to the knife, we believe, from its weathering, to be a species of steatite; but it has at first sight greatly the resemblance of compact felspar. This rock resembles one of the varieties of the summit of Moel Shabod, except that it wants the crystals of augite, manifestly imbedded in the latter.

Immediately after quitting the house of the guide on the bank of Llyn Cwellyn, we passed some large boulders fallen in all probability from Monydd Thevedo; they bore a considerable resemblance to the rocks of the summit of Moel Shabod, but we did not afterwards find any masses of the same variety *in situ* either in the ascent of Snowdon, or upon the mountain itself.

About the first two miles of ascent are along a horse road made by some Cornish gentlemen engaged in working a copper mine not very far beneath the summit of the mountain on the opposite side: by the displacement of the grass and thin covering of alluvium in forming this road, we could perceive that no other rock prevails along it, quite to the eastern ridge above *Cwm Clogwin*; nor did we perceive any other rock in situ vising through the almost unbroken verdant surface of this side of the mountain. These slates appear to be uniformly of the usual slate-blue colour, and not to differ perceptibly from the common variety of the killas of the Cornish miner; but just as we reached the ridge above Cwm Clogwin, the slates began to assume a different aspect and character, to become paler and more granular. We afterwards crossed a bare and somewhat elevated ridge of rock almost perfectly resembling some of the varieties of the base rock of Moel Shabod, being granular, and somewhat slaty, but sufficiently soft on its broken surfaces, which are very irregular, to yield to the pressure of the nail, and producing a very copious effervescence with muriatic acid. We consider this rock to be an intimate mechanical mixture of steatite and carbonate of lime, since the latter does not appear in separate masses or layers. Slates again prevailed, and then a rock somewhat slaty greatly resembling some fine-grained varieties of greywacke in its external character, but soft enough to yield readily in every part to the knife, and of which we consider the base to be steatitic: it also effervesces. That which succeeds this rock is a slate, yielding easily to the knife, and even, though with some difficulty, to the nail in its moist state. It partakes greatly of the nature of the slate just mentioned as occurring at the foot of Moel Eilio, but more nearly approaches common slate in colour and general aspect.

It must here be observed, that the cleavage plane of these slates and slaty rocks is nearly vertical, and in the usual direction of NE and SW; and that our walk was precisely across the cleavage at every step. Here is a small slate quarry. The guide told us that we had advanced just half way up the mountain.

A vast heap of ruin succeeded, consisting of much harder rocks than any we had before seen on the mountain, but many of them agreeing in character with the prevailing rocks of the base of Moel Shabod, and among them some which occasionally appeared altogether homogeneous, sometimes slightly porphyritic, the base being of a greenish colour, very hard, scarcely yielding to the knife, and containing minute crystals of felspar. This rock perfectly resembles the hard nodules already described as occurring abundantly in the steatitic rock near the foot of Moel Shabod opposite to the back of Capel Curig Inn. These often assumed the form of short irregular columns. Nearly vertical slates (as regards their cleavage) succeeded, and then a nearly vertical and somewhat slaty rock, which is hard enough to scratch glass, is translucent on the edges, and by transmitted light appears to contain minute specks of a green substance, probably chlorite, but superficially it appears homogeneous.

Still ascending the eastern ridge of Cwm Clogwin, we found slaty rocks again prevailing (the cleavage being indistinct and occasionally somewhat curved), but very soft, of a dark-green colour, and manifestly steatitic; the weathered surfaces show white specks, some of which yield to, while others resist the knife; the former consist of calcareous spar, the latter, judging by their fracture in the interior of the rock, and which, therefore, had not suffered any change by exposure, consists of steatite mechanically mixed with siliceous matter, since they resemble the larger masses included in the steatitic rock at the base of Moel Shabod; and being of a somewhat greenish colour, are not always distinguishable on the surface. To these succeeded a very imperfectly slaty rock, of a greenish colour, the weathered surfaces almost perfectly white—an appearance not very uncommon to the softest varieties of the more completely steatitic rocks. A still softer variety afterwards occurred, apparently consisting of a mechanical mixture of chlorite and steatite, and containing white specks of carbonate of lime. Rocks of the same description continued to prevail up the ascent of Cwm Clogwim, the upper part of which is not less than 2500 feet above the sea. The most remarkable of these consist chiefly of slaty chlorite, yielding, however, a brisk effervescence by the application of acid, though carbonate of lime is not apparent even through a glass.

As we approached the summit of Snowdon, keeping close to the ridge called Widdfa, we found it to consist entirely of several of the abovementioned rocks and slates repeated without any appearance of order in their recurrence, and with their cleavage plane (being all more or less slaty) nearly vertical, and in the direction of about NE and SW. In some of the porphyritic varieties, the slaty substance, when the softest of the two, had decomposed, leaving opaque white masses resembling those of the same rocks above Cwm Clogwin, projecting above the surface, while in others the imbedded substance (calcareous spar) had passed away, leaving the slaty rock vesicular. Carbonate of lime enters into the composition of the slaty part of both these varieties since they both effervesce in acid, but no quartz is perceptible in them. The substance of the rock is manifestly slaty chlorite. A variety of this description occurs interstratified on the very summit, with an apparently homogeneous and very soft, yet brittle, slate of a greenish colour.

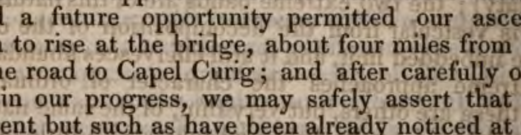
It has been known for some time that the impressions of a peculiar shell occur in considerable abundance within a few feet of the summit of Snowdon, and the rock enclosing them has been termed greywacke. We are decidedly of opinion that not a single rock occurs on Snowdon, nor as far as our observation extended, near that mountain that is at all allied to greywacke, unless the blue slates already noticed as prevailing up the ascent from Llyn Cwellyn, and others bearing the same characters, can be so considered, unaccompanied, as they certainly are, by *greywacke itself*, and often interstratified with rock of a *decidedly steatitic*, or of a chloritic base. These shells occur in slaty rocks, and in slates which are interstratified; the slates are of a greenish colour, the cleavage being less perfect than that of ordinary slates, and thin portions of them are green by transmitted light: they sometimes include whitish particles, which yield to the knife, but not to acid, and which we, therefore, conclude to be harder portions of the same substance as the rock itself, and of contemporaneous origin; and in this respect resembling the slaty rocks with which they are interstratified, and which also contain the impressions of shells. These rocks are sometimes porphyritic from the same cause as the slates, and being extremely soft, yield readily to the knife, and consist chiefly of steatite intermixed, and generally coloured by specks and layers of chlorite, which sometimes has been decomposed, leaving cavities: the steatite, however, is often of a yellowish colour, and somewhat translucent. That the softness of these rocks is not owing to the progress of decomposition is manifest from the inspection of the interior of vast blocks, occurring not on this mountain alone, and which have by accident or design been cleft asunder.

The rocks above enumerated were observed *in situ* as they appeared on the surface, or rather on the very edge of the precipices along the ascent of the mountain; and we conceive both from their examination individually, and from the circumstances of their connexion, that they are to be considered as varieties of the same rock: most of them pass into each other, some even in the course of a very few inches NE and SW. Thus what appears in one place as an almost homogeneous slate of a pretty close texture is sometimes so altered in its character in a short distance as to include multitudes of small white particles, which, when they effervesce in muriatic acid, we consider to be carbonate of lime, but when they do not, they appear to be of contemporaneous formation with the rock itself, and to consist of steatite intimately mixed with siliceous matter; and the character of the rock alters in becoming closer grained and less slaty in structure. It is certain, however, that in other places the interstratification of large masses of the rocks already described with slates is very manifest; for owing to the greater hardness of the rocks, and their less liability to decomposition than the slates, they frequently form ridges which rise uncovered by herbage above the slates on either side of them, and are seen projecting beyond the slates down the precipices of the Widdfa, and of that over Cwm Clogwin, while the indentations made by the decomposition of the slates (those which appear the most perfectly homogeneous yield soonest to atmospherical action) are covered by herbage, as is apparent every where from the very beginning of the ascent from the banks of Llyn Cwellyn.

In our descent beneath Crib Coch, and thence to Llanberris Pass, we observed no other rocks than such as have already been described: the run of the slaty cleavage is of remarkable uniformity every where.

Not very far beneath the summit of Snowdon are the workings of a copper mine still carried on. An examination of the refuse thrown out by the miner produced no rock that is not manifestly connected in character and composition with those found on the surface. The principal part of the rock immediately connected with the ore is slaty, of a greenish colour, often includes multitudes of minute crystals of iron pyrites, yields easily to the knife, is translucent on the edges, and it is evident by transmitted light and the assistance of the glass, that the colouring matter of the rock is a green substance, arranged in irregular lines, and we are confirmed in the opinion of its being chlorite, from having found masses of slaty chlorite enclosing yellow copper ore, and veins of quartz. We are not enabled to state any thing respecting the veins of this mine, not having been so fortunate as to meet with the captain, either on the mine, or at his residence in Beddgelert.

During our descent, we observed on the summit of the ridge which unites the Lluwydd (Cleweth) with the Widdfa, the summit of Snowdon, an appearance of stratification which, as viewed in the distance, may be termed basin-shaped, and which from its complete disagreement with all that we had hitherto observed, seemed to justify the conclusion that some important difference existed in that place in the nature of the rocks. It may be represented by the following rough sketch:



This remarkable appearance excited the wish to examine the spot, and a future opportunity permitted our ascent to it. We began to rise at the bridge, about four miles from Beddgelert, on the road to Capel Curig; and after carefully observing the rocks in our progress, we may safely assert that no rock was apparent but such as have been already noticed at the base of Moel Shabod, and on Snowdon. On examining the spot above alluded to, we found the rock to be essentially of the same nature; but being of a darker colour, and as it should appear containing more iron, it had become more subject to decay, producing on the surface an ochreous-brown colour very observable at a distance; and by a comparison of it with other rocks of a similar nature in other places, might be said to be considerably brittle. Still, however, it remains for us to account for the decomposition taking place in so unusual a form, and this, as we conceive, we are enabled to do from the observation of a fact, equally remarkable and unexpected.

In a country in which every appearance of actual stratification is parallel to the cleavage of the slates, we certainly should not expect to find the very rocks thus interstratified exhibiting characters which are to be accounted for only on the supposition of their having been deposited in the opposite direction, namely, nearly, but not quite, horizontally. In descending the Widdfa, and near the summit of the Lluwydd, we saw many rocks *in situ*, and even in very large masses, which exhibited nearly horizontal and alternate projections and depressions on their sides, and we found that these uniformly consisted of layers of varieties of the same rock, differing both in colour, and sometimes even in composition, the slaty cleavage being uniform and nearly at right angles to the direction of the layers. Those which were of the lightest colour, and were, therefore, judged to contain the least proportion of iron, being least subject to decay, protruded; while the ends of those which were darkest, having suffered the most by exposure, formed the indentations. These effects commonly take place on the large scale, but we were so fortunate as to find more than one instance in which a cabinet specimen completely illustrates the fact, each thus forming in itself a sort of epitome of the rocks of this region. We shall, therefore, attempt to describe one of the specimens in question, first noticing the fact, that these rocks do not possess a cleavage parallel to the direction of the variously coloured layers.

The base of the whole mass, which is about four inches long, and three wide, is manifestly steatite. It yields every where to the knife, affording a white powder. It consists of about 20 bands or layers in a direction not quite at right angles to the cleavage plane, varying from a yellowish-grey colour (when it is considerably compact) to a dark-green, which, however, is much heightened by the addition of moisture, and which arises from the intermixture of abundance of chlorite (it is then more completely slaty); the two extreme bands are of this nature, but one of them is vesicular from the decomposition of the calcareous spar once imbedded in it, and it still effervesces abundantly; the other extreme approaches the character of ordinary slate: between these are others, but very thin, of the same nature lying between others of a yellowish-grey colour with little or no intermixture of chlorite. Another specimen affords the opportunity of observing the very different effects of atmospherical action on the differently-coloured bands. This is much softer than the former in every part, and consists of, perhaps, 50 grey and dark-green layers varying from the tenth to the fiftieth of an inch in thickness: the dark-green have decomposed, leaving the grey ones protruding nearly half an inch.

It has been observed, that the cleavage plane of the slates in this district is not quite vertical, and the direction of the differently-coloured layers not quite horizontal; but they are never at right angles to each other; for it uniformly appears to be the case, that if it were possible to divide the latter along the lines separating the differently-coloured parts from each other, and all along the cleavage in the direction of the slate, we should reduce them into rhombic masses, agreeing in form with those often observable in slates, owing in the one case to a species of natural cleavage, or in the other to the progress of decomposition.

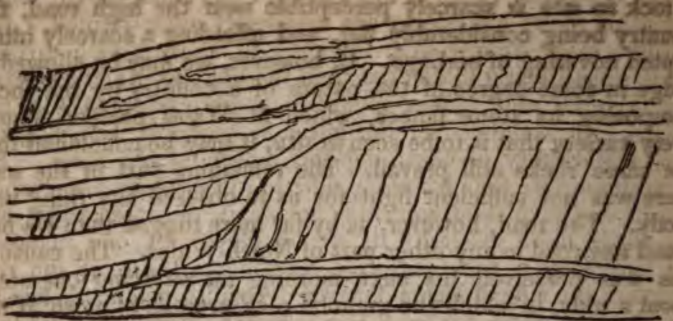
A consideration of the nature of these specimens just described, and more especially the inspection of the masses exhibiting the same effects on the large scale, amply account in our estimation for the appearances of stratification on the summit of the ridge connecting the Lluwydd with the Widdfa. It would account at least for the resemblance of a nearly horizontal stratification; and from examination of the spot, we are inclined to believe that the appearance of the dip and rise of the seeming strata is to be attributed only to the actual inequalities of its surfaces. A basin offering the same appearances of stratification occurs at the head of Llyn Idwell, in a branch of the Glyder Mountain, which we could not visit, but have no doubt they may be attributed to the same cause.

The continuation of our walk beneath the summit of the Lluwydd for about a mile, and a quick descent at its termination into Cwm Llan, offered no change in the nature of the rocks. The upper part of the descent of the Cwm is composed of blue slates, but near the bottom appears a long and thick ridge in the direction of NE and SW of the rocks so often noticed as occurring abundantly at the base of Moel Shabod; these continue to the foot. The dip of the slaty cleavage on the whole of the descent is towards the NW at about the usual angle of 54°. The appearance of roundness on the summit of the rocks heretofore noticed is remarkable in this neighbourhood.

Not far beneath the summit of the Lluwydd, which is 3000 feet above the sea, is a copper mine, the principal vein, according to the information of an intelligent miner, runs about NE and SW, and is about 18 inches wide. It possesses several strings or leaders, on some of which they were working; these did not appear to have any regular walls, and seemed to run nearly N and S, and to consist of a multitude of small strings traversing a rock greatly resembling that already described as being chiefly visible among the rubbish of the Widdfa mine, and, like it, containing minute crystals of iron pyrites.

In passing along the road from Capel Curig to Beddgelert, we perceived no rock with which we were not already acquainted; and for about two miles north of Beddgelert towards *Carnarvon,* only the same varieties occurred. The same observation applies to rocks of the Pass of Pont Aberglaslyn, where the same rocks and slates are manifestly interstratified. Just beyond Pont Aberglaslyn, the road to Tan y Bwlch divides; the new road being more even than the old one, which traverses the elevated and barren mountain region between the two places, we walked over the latter as probably affording the best opportunity of observing the nature of the rocks. For several miles we found nothing different from those every where observed, except that, generally speaking, they are harder, and the slates more generally incline to blue. In one instance, we perceived a rock perfectly resembling those of the ruin covering the side of Penmanmawr. A short distance before we began to descend towards Tan y Bwlch, however, a change was perceived; ridges of rock in which no steatite was observed were interstratified with the slates parallel to their cleavage plane, and these running in the direction of NE. and SW., and apparently consisting only of fine grained chlorite, calcareous spar, and quartz, the latter prevailing; one ridge appeared to consist of granular quartz including a very few specks of chlorite. These rocks rose in ridges considerably above the slates bounding them on each side, and with them dipping towards the NW. at the usual angle of 54°.

Slates prevail on both sides of the vale of Festeniog below Tan y Bwlch Inn towards the sea, the flat base of the vale consisting apparently of alluvial matter: the beauty of this vale, however, which is justly celebrated, appears to be owing chiefly to the decomposition apparent in the slates wherever they are visible, forming a soil in which the woods of its northern bank flourish luxuriantly. A part of the hill on the opposite side is clothed in like manner, but the trees are of less height, the soil in which they grow being also a slaty rock in part decomposed, or in fragments generally so very small that we could not perceive the direction of the cleavage plane. On the summit of the hill, however, just before we began the descent to a waterfall termed the Rhyader Dhû, about two miles from Tan y Bwlch, the rock did not differ from those of the base of Moel Shabod, and is interstratified with slates: a fallen mass appeared in the descent to the waterfall consisting of the same rock enclosing round or ovate masses of a quartzose substance. In the basin of the upper waterfall, we observed a new appearance in the arrangement of the rocks so often noticed. The slates here appeared in nearly a horizontal position, and exhibited a tortuous course, which we have never before observed, while the rocks lying between slates assumed a somewhat columnar form, thus:



We afterwards traversed in our way to Harlech, a country consisting of the usual rocks and slates, possessing but little vegetation for some miles. Quartz often prevailed so greatly in the rock as to give it a character approaching to that of quartz rock. The precipice overhanging Llyn Tegwin, which is very lofty and unusually rugged, consists chiefly of slates presenting a rhombic form, probably from weathering, the acute angles of the monstrous rhombs protruding from the face of the precipice. Two layers of considerable thickness of the usual rock were interstratified parallel to the cleavage plane of the slates, the one near the summit, the other at the base; they appeared parallel, the direction of the latter being towards the north-west at an angle of 30°, being a much lower angle than we had before seen.

At about the distance of four miles from Harlech, and while traversing a valley chiefly, if not altogether, of slates more than usually brittle, and apparently in a state of decomposition, and admitting of the growth of considerable woods of small oaks, we perceived on our right hand two lofty hills, each presenting to us a rugged scarp, consisting of slates, having precisely the same appearance as those overhanging Llyn Tegwin, and crowned, one of them in a remarkable manner, by rocks in the form of closely aggregated columns, of the height, as well as we could judge from below, of 30 to 40 feet. These rocks dip to the NW, and though we had no opportunity of inspecting them, we inferred from what has been described as occurring in the ample basin of Rhydr Dhû, that they did not differ from the usual rocks of the district.

The long ridge, at the southern termination of which Harlech Castle stands, and which presents its side towards the sea, consists, in its upper beds, of a coarse slaty rock manifestly appertaining to the rock so constantly observed, of which its lower beds consisted; quartz, however, prevailing in it in a more than common degree.

In the first three or four miles from Harlech towards Barmouth, a rock *in situ* is scarcely perceptible near the high road, the country being considerably flat, and affording a scarcely interrupted covering of herbage. If, however, we may be allowed to judge in some degree of the nature of a country by the rocks composing its stone fences, which in Wales present almost every variety that is to be seen *in situ*, it may be concluded that the same rocks still prevail. The remaining part of the way, there was not sufficient light for us to observe any thing correctly. The road, however, is by far more rugged than we had found any road in any other part of North Wales. The cause of this was perceived the next morning on retracing our steps about a mile, in which the granular rock prevails in most, if not all, the varieties observed at the base of Moel Shabod. In some places, however, quartz entered into its composition in greater abundance than in any rock perceived by us on that mountain.

On quitting Barmouth for Dolgelly, we found that the very last house stands on the termination of the granular rock, and that the slates perceptible on the hill behind the town descend to this place, and repose on the rock in a line perfectly consonant with the plane of the slaty cleavage, which as usual runs NE and SW, and dips towards the SE at an angle of 68°. The slate here is somewhat coarse, though with a completely slaty structure: it is translucent on the thin edges, and of a green colour by transmitted light.

Slates continue to prevail along the road from Barmouth to Dolgelly, and they are in many places newly laid open by the recent widening of the road, and for materials for its repair. The direction of the laminæ continues, as we observed it immediately on quitting Barmouth, the whole of the way with the same dip, the angle somewhat declining in approaching Dolgelly.

But though slates prevail in the mountains on our left from the base to the summit, we were able, in several instances, to perceive, before our arrival at it, a change in the nature of the rock, by the appearance up the side of the mountain of a ridge differing manifestly from the slates, and which, on examination, proved to be a rock of the same nature as the base rock of Moel Shabod, but often without any appearance of quartz or chlorite, and interstratified with the slates in the direction of NE and SW. The rock of the only ridge which we carefully examined, appears to consist of steatite occasionally mixed with carbonate of lime, since it effervesces in patches. It is fine-grained, yields readily to the knife, of a greyish colour, and translucent on the edges.

In approaching Dolgelly, we had a good view of the summit of Cader Idris, and of some ranges of lower mountains, which may be considered as forming a part of its northern side. These possess a character different to that of the summit of the mountain in being much whiter, and more rugged ; while in the distance, the line of their extended summits is more nearly horizontal than those of any other mountains we had seen ; and there are still lower ranges, apparently consisting of the same rock, running parallel with them towards the sea coast. It is undoubtedly difficult to determine the precise run of these ranges in the circumstances under which we viewed them, but we judged by the bearing of the compass, that the run of their ridges is in the direction nearly of NE and SW ; that is, parallel to the direction of the cleavage plane of the slates here and almost every where else.

In our route to Machynlleth, we crossed Cader Idris, descending by Craig y Caie to the Mynfedd Inn ; and being aware that it is the intention of Mr. Aikin, ere long, to present to the public a detailed account of the geology of this mountain, we abstain from more than a general remark or two, viz. that many varieties of the rocks prevalent at Moel Shabod appear likewise in this mountain, the escarpment of which towards the north consists of columnar rocks, bearing generally more completely the character of greenstone than any rock we have observed on the north of that mountain ; that on the face of this escarpment are visible two or more beds of slates, interstratified with the columns of greenstone, the slates resting on the summits of the lower columns, while the bases of the upper rest upon the slates ; that the plane of the slaty cleavage is about at right angles to the position of the columns, running NE and SW, and dipping at about the angle of 68° to the SE.

In ascending the mountain, we found several impressions of a shell differing from those observed on the summit of Snowdon, and also from those found near the base of Ben Glog and at Moel Shabod, in a rock which is fine-grained, soft, apparently composed chiefly of steatite, and perfectly resembling a variety occurring at the base of the latter mountain, and in many other places.

The road to Machynlleth runs chiefly along narrow vallies often bounded by lofty and steep hills. For the first two or three miles, their sides, though verdant to the very summits, are neither well wooded nor cultivated, and wherever a rock became visible, it was always slate, nor did any solid rock appear during the whole of the route to Machynlleth, either by the sides of the road, or in the form of ridges as heretofore ; the stone fences are of slate, and the road being composed of the same material, is, though hilly, superior to most we had lately travelled. After the first two or three miles from the Mynfedd Inn, extensive woods of young oaks covered the sides of the mountains often nearly or quite to their summits, almost the whole of the way to Machynlleth ; while here and there the oak, the ash, and the sycamore, were of a considerable size ; the appearance on either hand forming a perfect contrast to the general scenery of

Caernarvonshire; as, for instance, the greater part of the route by the old road from Pont Aberglasslyn to within a mile or two of Tan y Bwlch, which chiefly traverses the solid rock.

The slates between the Mynfedd Inn and Machynlleth have the same dip and direction as those on the escarpment of Cader Idris. The houses of Machynlleth are all of slate.

The country between Machynlleth and Aberystwith partakes of the same general character. Lofty hills, with steep and rapid slopes; and bounded by narrow vallies, frequently pretty well cultivated, the herbage covering even the summits of the hills, which are rounder than before. Woods of small oak, but of considerable extent, were frequent on the sides even when very steep. The rock rarely appears through the verdure, so that almost the only chance of gaining information respecting its nature is to be found in the little quarries beside the road, or where, as is sometimes the case, the unbroken rock constitutes the road itself. Many instances of this occurred soon after leaving Machynlleth, affording the opportunity of ascertaining that the direction of the cleavage, and the dip, still continue the same. The rock, however, is not always a pure slate, since it is very commonly interstratified, as every where else, parallel to the direction of its cleavage, with thin layers of a granular stone, in its external character greatly resembling some of the varieties every where observable in the more northern mountains, but of a colour more nearly approaching that of the slate, and often consisting of variously coloured particles lying parallel to the plane of interstratification.

We regret omitting the opportunity of examining the slate quarries of Aberystwith. We walked, however, to the bold projecting rocks at the point on the NW of the town, where we found almost the only instance of irregularity in the dip, and contortion of the strata, that we have observed. At the point, and as far as we could see the coast beyond it on the north, the dip appears to be nearly E at a low angle, although that of the rocks at its foot, which are covered at high water, dip in another direction; and during the very short time we looked at these rocks, it appeared to us that those immediately in contact with the point, towards the town, and which are contorted, dip in a third direction; but it is not improbable that further investigation might have proved the inaccuracy of some of these appearances. Every where the slate is interstratified parallel to its cleavage plane with a more or less granular rock, much resembling some of the finer-grained varieties of the base of Moel Shabod.

The same kind of country continued to the Devil's Bridge, and very slight opportunities of examining the rocks on one side only of the road, afforded us only the information that the summits of the hills are of slate, often in very small rhombic pieces, as though, from some cause or other, they had been shattered.

Close to the Devil's Bridge, on the side beyond the Inn, there is a quarry of slates, of which the cleavage runs as usual NE and SW, dipping towards the NW at 50°; but on the northern side the rock immediately adjoining the bridge, the dip and direction are very different. It is, however, manifest that this mass, part of which forms the abutment of the bridge, is not *in situ*, since its seams are opened, and the rock itself appears to have been shattered in its fall. In the deep ravines of the singular scenery opposite to the Inn, we anticipated an opportunity of ascertaining the nature of the rocks of this place. On descending these ravines to view the fine waterfalls beneath the bridge, we found the dip and direction agree uniformly with those of the quarry above-mentioned. We afterwards went to see the grounds belonging to that boast of all the tourists, Hafod, and on the banks of the Ystwith, traversing its beautiful valley (in which it appeared to us that art had done much, but nnture more), we had numerous opportunities of ascertaining that the dip and direction of the slates agree with those near the Devil's Bridge. In both places, the slates still continued to enclose layers of a granular rock resembling that so often noticed. Occasionally also, it occurs in blocks and kernels, and is so soft as to yield easily to the knife, and in several instances was observed to decompose in the same cellular manner as some of the chloritic slates of the summit of Snowdon and other places, the cells, however, being much smaller. Near the bridge at the termination of the Hafod grounds on the road to Tregarrow, were some large masses of slate enclosing layers of the rock in question, parts of which were so far decomposed as readily to break down into a perfectly soft substance of an ochreous-brown colour. Hitherto, therefore, the slates and slaty rocks appear to partake largely of the characters of those forming the more mountainous regions of the most northern parts of Wales; and it may be observed that hitherto we have not seen a single rock bearing in any degree the character of greywacke.

Between Hafod and the Devil's Bridge, the country continues, as between the latter place and Hafod, bold, but extremely sterile, the lofty hills being so completely covered by coarse verdure to their summits, that the rock is visible chiefly along the water-courses in the bottoms of the valleys. Few trees, and as few attempts at cultivation, are visible on the sides of the road. Slates enclosing masses and layers of the same varieties as those prevailing between Machynlleth and the Devil's Bridge are occasionally to be observed, and possessing the same line of bearing.

Just before arriving at Rhyader, however, the character of the country became changed: the hills surrounding that place are lower, and have rounder summits than any that we have previously observed; while the broader valleys offered the reverse of the picture we had so lately seen, being well cultivated and wooded. We now turn into the valley of the Wye, and

About a mile south of the town, an abrupt cliff of considerable height attracted observation from the nature of the rock, which is in pretty thick beds or layers, dipping to the NW about 35°, and consists apparently of minute portions of crystalline quartz firmly adhering, and presenting innumerable small ochreous specks, as though some one of its constituents had suffered decomposition, and thus constituting a paste which included somewhat round (perhaps rolled) masses of granular quartz and of hornstone (?) of considerable size, and here and there small transparent crystals of felspar. This rock altogether greatly resembles some varieties of the more compact and quartzose beds of the old red sandstone.

The appearance of a rock possessing characters so greatly differing from every thing that we had seen in North Wales, indicated a complete change in the geological features of the country. This, however, did not altogether prove to be the fact, for we afterwards observed repeated instances of the same slates, and included rocks, as had been noticed before; but still it appears to us that an investigation of this part of the country, and particularly of the immediate neighbourhood of Rhyader, would prove of great interest to the geologist, as affording him the opportunity of observing rocks of very different characters in a very short compass, in such a manner, as to prove their connexion and possible transition from the one into the other.

Between Rhyader and Built we also observed a rock most essentially differing from any of the preceding. It has the appearance of an indurated clay, which sometimes appears in layers, including masses of the same substance, which, by exposure, open concentrically, and finally break down into a clay; and it is only on the assumption of the prevalence of this indurated clay to a considerable extent, that we are enabled to account for the appearance of some large and high commons in this route, having pretty level surfaces.

About a mile on the north of Built, we observed in the bed of the river a rock which may, perhaps, be the greenstone noted in Mr. Greenough's map as belonging to the coal formation. It is an extremely fine-grained rock, nearly black, traversed by veins of quartz, but the component materials of the rock itself are not discoverable by the help of a glass.

In the bed of the river on the left of the bridge, on entering Built, we observed a shale much resembling that of the coal formation, containing large spherical masses, often in the form of septa, of a substance which is very ponderous, and considerably resembling that of the septaria enclosed in the London clay. It also contained impressions of vegetables.

After leaving Built towards Brecon, the country still continues to improve in fertility, and the hills are lower, but still few openings appear, and scarcely a rock is visible above the surface.

Within a mile of Built, the soil begins to be tinged of a reddish colour, and about three miles from it we observed a small quarry on the top of a hill, and near the road side, situated in a sandstone perfectly resembling the old red. It afforded two or three varieties; one of them consists apparently of an indurated clay or marl of a red colour, enclosing specks of mica; another of grains of siliceous sand, and an ochreous substance connecting them; a third resembling the sandy variety, except that it was rendered slaty by the intervention of close layers of mica; these varieties are interstratified, and dip at about 15° to the SW.

In conclusion, we have to observe, that previously to our quitting the hospitable roof of Mr. Dawson, at Bangor, he informed us that some varieties of the rocks of the district we visited had been pronounced by certain French geologists to be the *steaschiste* of Brongniart. Since committing the foregoing pages to the press, we have consulted the description by that eminent mineralogist of the Geology of the Cotentin inserted in the 35th volume of the Journal des Mines, and his particular description of the steaschiste to be found in his "Essai d'une Classification Mineralogique des Roches mélangés," in the preceding volume. The perusal of these at once convinced us of the relation existing between the rocks of North Wales and those of the Cotentin, and even of their identity in so far as related to their actually consisting of the steaschiste, and of that alone; for we did not perceive any rock whose character sufficed to raise a doubt of the whole being of one formation. We refer the reader to the two memoirs above cited, the perusal of which will readily satisfy him of the correctness of our present views of the nature of the rocks in question, and that their proper and expressive designation is steaschiste; of which we have described most of the varieties mentioned by Brongniart: they are as follow: Steaschiste *rude; porphyroïde; noduleux; steatiteux; chloritique; diallagique* (ours is rather *augitique*); *ophiolin; phylladien.*

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Having sent to Mr. G. B. Sowerby, of King-street, Covent-garden, all the impressions of shells found by us in Wales, and requested of him some remarks upon them, which his intimate acquaintance with conchology well qualifies him to afford, we annex the communication received from him on the subject, first observing that the impressions figs. 1, 2, 3, 4, 5, and 9 (Pl. XVII), are from the summit of Snowdon; 6, 7, 8, 10, 11, 12, and 15, are from the road side near Pont y Cyffin; fig. 13 is from about midway between the Devil's Bridge and Rhyader; fig. 14 from Cader Idris.

Fig. 15.

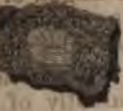


Fig. 10.

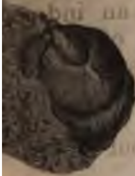


Fig. 13.

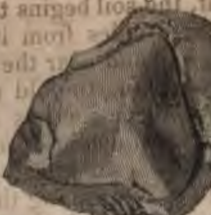


Fig. 11.

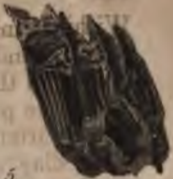


Fig. 4.

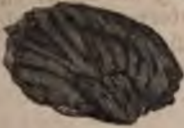


Fig. 5.

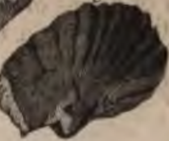


Fig. 14.

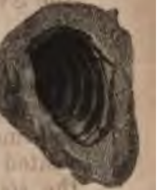


Fig. 9.

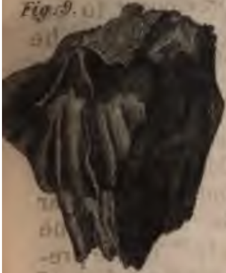


Fig. 1.

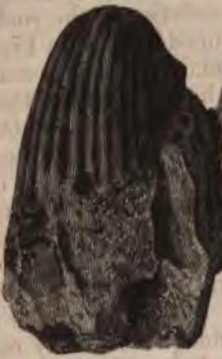


Fig. 2.

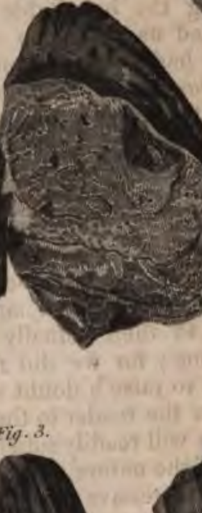


Fig. 8.

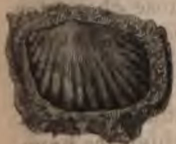


Fig. 12.

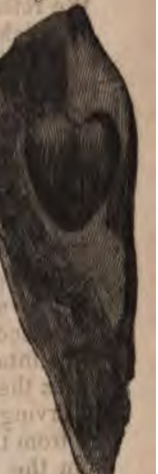


Fig. 3.

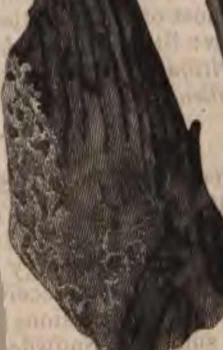


Fig. 6.

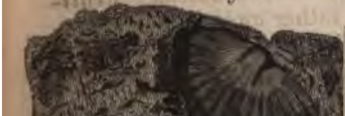
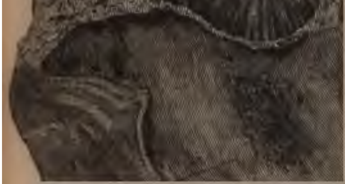


Fig. 7.



J. Shar

G. B. Sowerby del.t

Engraved for the Annals of Philosophy _ for Baldwin, Cradock & Joy. Decr 1. 1822.

GENTLEMEN,

In conformity with your expressed wish, I forward to you the accompanying drawing (Pl. XVII) of the fossils from Snowdon and its vicinity; and shall now proceed to offer you my opinion, or rather my conjectures, upon the nature of each one in particular, first observing that scarcely any of them possess sufficient character to enable me to speak with any tolerable degree of certainty. Several of them must remain undecided, until more perfect specimens can be obtained; because they are destitute of those parts from which generic characters are taken. Almost all the organic remains to be traced in these specimens appear to me to be bivalve and principally terebratuloid shells. The specimen numbered 1 and 2 in the drawing appears to be a cast of the inside of the deep valve of a *Productus*,* of which fig. 1 shows the back, and fig. 2, the hinge: it has distinct but rather flat ribs, and it is compressed in a direction from the back to the hinge. Fig. 3 is a view of another specimen, which I believe to be the same species, but which is compressed laterally, so that the ribs are much more prominent. Figs. 4 and 5 are two views of an entirely detached little cast which is rather concave on one side, and convex on the other. I think this may decidedly be referred to the genus, if indeed it be a distinct genus, described under the name of hysterolites, which we are informed in the Dictionnaire des Sciences Naturelles are only found in the *oldest* beds. As a species, it differs from the only one I had before seen in having distinct longitudinal diverging ribs. Fig. 6 is a representation of a fossil which I thought at first was probably the flat valve of a productus; but judging from its principal features, I am now rather disposed to think it may also be an hysterolite. It is a very flat impression, and it has two sets of diverging ribs, only distinct towards the margin, one set smaller than the other, and interposed between the larger. The fragments represented in fig. 11 appear to me to belong to the same; they are compressed in various directions. There is another impression upon the same stone as fig. 6. I have numbered it 7, and I cannot help expressing some doubt about the real nature of this impression; if it be that of a shell, it is certainly an impression of the outside of an avicula. Fig. 8 is probably the impression of the outside of the opposite valve of the same kind of shell as fig. 6. The fossil represented at fig. 9 is, perhaps, the most singular of all; it appears to be a cast of the inside of a terebratuloid shell, and like many of them it has several strong diverging ribs, most prominent towards the margin; but it is principally remarkable for a strongly prominent three-sided projection, something like an irregular, ill-shaped tetrahedron, one point of whose base is exactly at the point of the *umbo* of the hinge, and the two others are directed towards the two corresponding sides of the margin. I strongly suspect that this projection has been rendered unnaturally prominent by being rather laterally compressed. Fig. 10 is an indistinct fragment of the impression, probably of the lower valve of an hysterolite. Fig. 12, a bit of slate, which contains some pyrites, and upon which is an impression very much like that of the scale of the cone of some species of *pinus;* but it is quite impossible for me to decide whether it is an animal or vegetable remain. Fig. 13 has the appearance of the outside of a bivalve shell, but it is extremely indistinct, and I dare not venture a conjecture upon it. Fig. 14, part of an impression of the outside of one valve of a bivalve shell, but to what genus it is referrible, I do not know; probably a Venus or a cytherea. Fig. 15, a very indistinct section of a madrepore. I am, Gentlemen, yours, &c.

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* I make use of this name because it is at present generally adopted. I am perfectly aware of the impropriety of using a Latin adjective as a generic appellation, and am consequently happy to learn from a gentleman who has lately taken much trouble in investigating these fossils, that the use of this appellation will be superseded, and a name which has the right of priority, adopted in its stead. In using the term Productus, I do not venture an opinion upon the real nature of these fossils.

G. B. SOWERBY.

ARTICLE II.

On the Ultimate Analysis of Vegetable and Animal Substances.

By W. Prout, MD. FRS.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Southampton-street, Nov.* 15, 1822.

IN the second part of the Phil. Trans. for the present year, just published, there is a paper by Dr. Ure on the Ultimate Analysis of Vegetable and Animal Substances. In this paper, Dr. Ure states that he has constantly found about three per cent. more of carbon in sugar than what I obtained, and that the results of his analysis of urea differ very considerably from M. Berard's and mine, especially in the proportion of azote. The chief object of this notice is to endeavour to throw some light on these differences; and first with respect to sugar. Dr. Ure states that he employed the best refined sugar of commerce. I used perfectly white and pure crystallized sugar-candy, under the impression that this would be more likely to be fixed and definite in its composition, than the imperfectly crystallized sugar in common use. I have indeed, with other views, once or twice operated on common sugar, but without much attention to accuracy; so that I cannot with certainty state whether my results coincide with those of Dr. Ure. With respect to urea, what I employed was perfectly pure, in which state it exists as a beautifully white crystallized substance (very like oxalic acid in appearance), permanent in ordinary states of the atmosphere, and without any remarkable taste or smell. I may remark, that the above two substances were among the first I analysed, and that the analyses were made with a charcoal apparatus much less capable of precision than the lamp apparatus which I subsequently employed.

The views which I published some years ago respecting the atomic theory, seem to be now generally known in this country. These views at the time led me to others which I was exceedingly anxious to verify; and as I was interested, for other reasons, in the composition of organic substances, it struck me that by submitting these substances to analysis, I might not only obtain a knowledge of their composition, but by investigating the laws which might regulate the union of their elements, hydrogen, carbon, oxygen, and azote, be able to obtain an insight into the laws which regulate the union of other elementary principles. With these views, therefore, I set to work, and after very great labour, and no trifling expence in apparatus, &c. succeeded, as I supposed, in analyzing more or less perfectly almost every well-defined and *crystallized* organic substance that I could procure. A few of my earlier results were published, perhaps, prematurely, but the great mass, as is well known to several of my friends, still remains by me, nor have I, for various reasons, the least inclination to publish them at present. In the mean time, however, it may be stated, that the substances analyzed were dried at 212° in vacuo with sulphuric acid, by means of an apparatus described by me several years ago for that purpose, that every precaution (including those mentioned by Dr. Ure as well as others), were taken to insure accuracy, that, with the exception of sugar, and one or two other substances, every substance analyzed by Dr. Ure and myself in common appears by the charcoal apparatus to contain *less* carbon than by the lamp apparatus.*

I am, dear Sir, yours, &c.

WILLIAM PROUT.

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* In making this remark, I by no means wish to insinuate that Dr. Ure's results are erroneous; my object is merely to show that the lamp apparatus is as capable in many instances of oxidizing carbon as the charcoal apparatus. It is probable that several of the substances examined by Dr. Ure could only be analyzed by some such means as those he employed; but for the analysis of most substances containing azote, I do not hesitate to say that I prefer the lamp apparatus.

ARTICLE III.

Remarks on the Geology of Lindisfarn, or Holy Island. By N. J. Winch, Esq. Honorary Member of the Geological Society of London, and of the Mineralogical Society of Dresden. With a Plate. (No. XVIII.)

(To the Editor of the *Annals of Philosophy.*)

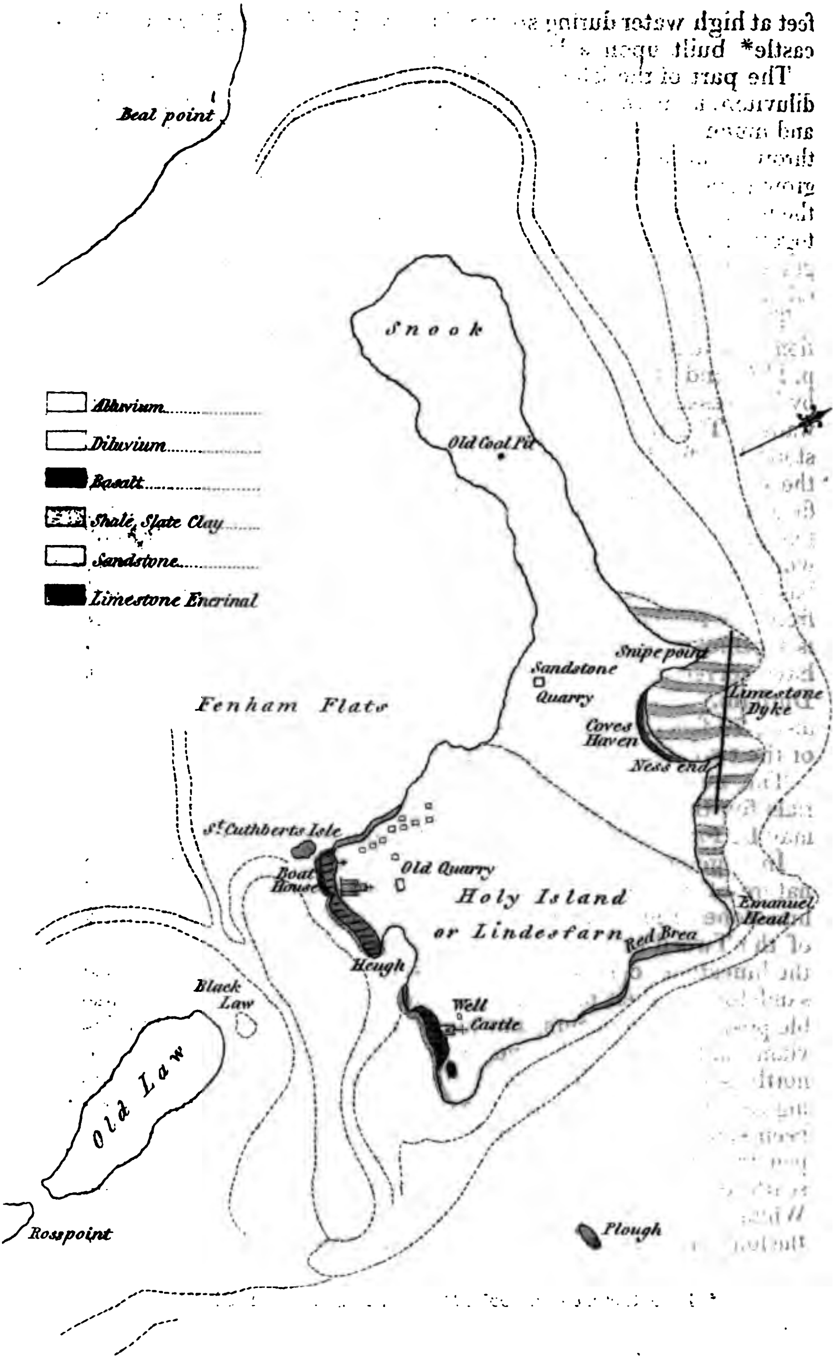
SIR, *Newcastle-upon-Tyne, Nov.* 1, 1822.

PREVIOUSLY to attempting a description of the geological structure of Lindisfarn, it may not be amiss to mention a few leading particulars respecting the island, which will at least save the trouble of referring to printed authorities on the subject. The venerable Bede, who wrote in the eighth century, calls Lindisfarn a semi-island, being surrounded by the sea twice every 24 hours; and a popular poet of the present day delineates this striking phenomenon in the following lines:

> "The tide did now its flood-mark gain,
> And girdled in the saint's domain;
> For with the flow and ebb, it still
> Varies from continent to isle;
> Dry shod, o'er sands twice every day,
> The pilgrims to the shrine find way;
> Twice every day the waves efface
> Of staves and sandal'd feet the trace."

Ages have passed away since the time of Bede, and but little alteration seems to have taken place during the long interval, either on the western side of Holy Island, or on the opposite coast of Northumberland—a clear proof of the sea having made no considerable inroads for centuries on the indented shore of this part of England, and warranting the supposition that the Farn islands and Staples must have been divided from the main land by the agency of a temporary current of water sufficiently strong to break up and remove the adjoining strata of limestone, shale, and sandstone, but not powerful enough to destroy the more obdurate masses of basalt which have been thus left in their present isolated situations.

The length of the island from north to south including a peninsula called the Snook, is about two miles and three quarters; its breadth from east to west, a mile and a half. The town contains about 500 inhabitants, of whom 70 are fishermen, usually engaged in the white-fish or herring fisheries, but acting occasionally as pilots, many of them being legally authorized by the Trinity House at Newcastle. The harbour is extensive and safe, except during heavy gales of wind from the westward; it has eight feet water on the bar at low water, and twenty-two



[illegible] Baldwin Cradock & Joy Decr 1. 1822.

feet at high water during spring tides, and is defended by a small castle* built upon a lofty basaltic rock.

The part of the island, as coloured (Pl. XVIII), covered with diluvium, is in a state of cultivation; and though the soil be light and much encumbered with stones, affords a good rent, the sea throwing up manure in abundance, and the demand for land to grow potatoes being considerable. The uncultivated portion of the island consists of a range of sand hills, or links, slightly held together by the creeping rods of the sea lyme-grass, sea mat-grass, rushy wheat-grass, and sea carex, and is occupied as a rabbit warren.

The ruins of the Abbey seem to be nearly in the same condition as when drawn for the print in Grose's Antiquities, vol. iv. p. 117, and are secured in some degree from future dilapidation by buttresses having been recently erected to support the outer walls. The masonry appears rude, yet the building has withstood the frosts and tempests of many centuries. The sides of the walls alone are constructed with hewn stone; the inside is filled by fragments mixed with mortar: in the former instance, fine-grained red sandstone, with a few courses near the top of white sandstone, has been the material used; in the latter, basalt, limestone, sandstone, or whatever else could be collected from the sea beach. With the exception of the chancel, which is Gothic, this Abbey is of Saxon architecture, and appears to have served as a copy for the more magnificent cathedral at Durham. Its dimensions are: length, 138 feet; length of cross aisle, 70 feet; breadth of the body of the abbey, 18 feet; breadth of the two side aisles, 9 feet each.

The monastery has been nearly demolished to afford materials for the erection of the present church, though some idea may be formed of its size and figure from the ruins still left.

In a geological point of view, Holy Island partakes of the nature of the neighbouring district, or is included in the encrinal limestone formation, which traverses England from the vicinity of the Tweed to Derbyshire. The rocky beds, associated with the limestone, consist of shale or slate clay, and red and white sandstones: their dip is south-east. Basalt in an unconformable position also occurs, and these are in part covered with diluvium, and in part with sand drifted from the shoals lying to the north (see Plate). That the latter forms but a superficial covering to the peninsula called the Snook is evinced by a pit having been sunk through it in search for coal; to what depth the miners penetrated I could not learn, but fragments of bituminous shale scattered about served to prove the nature of the substratum. While on the subject of alluvium, it may be right to notice, that the long shoal stretching from Goswick towards the north of the island consists chiefly of greywacke pebbles, washed down from the mountains of Selkirkshire, and deposited in their present situation by the current of the river Tweed. For this information, I am indebted to a friend,* thoroughly acquainted with the geology of the border. The diluvium covering the southern division of the island constitutes a tolerably fertile soil, though sand appears to predominate: mixed through it, are water-worn masses and boulders of granite, porphyry, syenite, greywacke, conglomerate, encrinal limestone, basalt, and sandstone, the produce of distant mountains, as well as of its own rocks.

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* Latitude of the castle, 55° 40·20′ N.; longitude, 1° 46·38′ W.

The centre of the island presenting but few points for geological investigation, it is my intention to commence by describing the rocks forming the cliffs and beach, beginning with the south side of the island, and passing along its eastern return by its western shores. By this mode of survey, a ridge or overlying mass of basalt will first come under consideration; it may be observed on the main at Kyloe Crags, taking a south-easterly direction, and again makes its appearance at St. Cuthbert's Island or Hobthrush, where its elevation is inconsiderable. At the western extremity of the Heugh, it forms a ridge 45 feet in height, by 120 feet in breadth, but at the distance of 500 yards is lost under water. Near the eastern extremity of the basin, or small harbour, it again rises in irregular columns to the height of 105 feet, and on these stands the castle. Further to the south-east, the stables were built on a similar rock; and finally the basalt may be traced in this direction to the Plough and other detached rocks visible only when the tide is passed half ebb. This basalt is generally of an iron-grey colour, and fine-grained texture, occasionally with specks of pyrites, but at the foot of the cliffs of the Heugh its fracture becomes earthy, and specks of calcareous spar are scattered through it. That this line of eminences is not a dyke protruding above ground is clearly proved by the basalt resting on limestone and shale at the north-western part of the Heugh; on shale, a little eastward of the path, which crosses that ridge; and again on limestone near the castle. Though the regular dip of the stratified rocks is to the south-east, yet the edges of the stratum of limestone where it comes in contact with the basalt, near the boat houses, rises rapidly, and in part rests against it; fragments of limestone appear also to be included in the body of that rock; but on the beach at an inconsiderable distance from the Heugh, the limestone follows its regular course. The colour of the limestone, where it approaches the basalt, is pale ash-grey, its texture crystalline, and it contains iron pyrites and small veins, turned red by the oxidation of their iron. At a distance from the basalt, the limestone is of a dark smoke-grey colour, splintery fracture, with veins of white calcareous spar, and the casts of the encrinal, fossil, and obscure bivalve shells, are imbedded in it. Four inches of highly indurated shale, of an iron-grey colour, and breaking into angular fragments, may be remarked between the limestone and basalt. A little to the east of the boat houses, shale, containing marine exuviæ, underlies the basalt, and at one point may be seen in the cliff, 20 feet above high water mark.

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* Matthew Culley, Esq. of Akeld.

To the eastward of the path which crosses the Heugh, shale or slate clay is the stratum on which the basalt again reposes. At the point of contact, the shale of the upper part of the bed is of an ash-grey colour, and its fragments are angular; it contains specks of calcareous spar, and is hard, when compared with the schistose layers under it. These are of a pale-grey, passing into reddish-brown by decomposition, and abound in casts of the following organic remains:

Helix cirriformis, Sowerby	T. 170,	f. 2.
Terebratula biplicata, ditto	T. 90,	f. 1.
Terebratula Wilsoni, ditto	T. 118,	f. 3.
Spirifer trigonalis, ditto	T. 265.	
Spirifer oblatus, ditto	T. 268.	
Productus longispinus, ditto ..	T. 68,	f. 1.
Productus Flemingii, ditto	T. 68,	f. 2.

Also a small bivalve, probably a Modiola, and a fossil, resembling a Belemnite, but not thicker than a quill, though of considerable length.

The bed of shale lying at some distance from this part of the Heugh, and covered by the sea at high water, is very hard, black, and encloses calcareous casts of the encrinal fossil, and cubic pyrites, and the organic remains before enumerated.

The basaltic eminence on which the castle stands is the most striking feature of the island, the summit of the rock being decidedly columnar: it is of the same nature as the Heugh, and rests upon shale of the same description; but on the beach immediately below the gate leading into the castle field, a small portion of very beautiful limestone may be noticed; its colour is pale reddish-brown passing into bluish-white; its lustre pearly; and texture highly crystalline, similar to the last mentioned limestone, when in the vicinity of basalt. The strata of limestone and shale along this side of the harbour from the Heugh to the castle, where exposed to view at low water, may be observed to possess an undulating form, the ridges rising at right angles to the inclination of the beds, but this phenomenon is more remarkable at the Coves.

From the castle point to Red Brea, the cultivated land is defended from the inroads of the sea by a barrier of boulder stones thrown up during easterly gales of wind. On examination, these will be found to consist of the same varieties of rocks as those imbedded in the diluvium. From this stony beach to beyond the cliff called the Red Brea, the stratum covered by the sea at low water is of coarse-grained yellowish-brown micaceous sandstone, becoming brick-red when in a state of oxidation. The Red Brea does not excede 30 feet in height, 20 of which consist of diluvium, two of coarse-grained micaceous sandstone, one of bituminous shale mixed with fragments of coal, two of sandstone like the former, and five of shale. The strata on this side of the island also undulate. Emanuel Head is what seamen call a green bluff; its extremity is protected by an accumulation of boulder stones. To the west of this headland, limestone and sandstone become the prevailing rocks, but the haven in which the coves are situated is the most advantageous point for observing their construction. The coves are recesses hollowed out in the soft sandstone of the perpendicular cliff by the action of the sea and the weather, their harder covering having withstood these powerful agents. The principal cove is supported by two natural pillars, by which its entrance is divided into three pretty regular arches, the centre one being much the largest. The cliffs here, including their covering of earth, are about 40 feet high: the first bed of limestone is four feet thick, of a pale ash-grey colour, containing the encrinal fossil and bivalves, and breaking into cubic fragments; it is divided from the second bed by eight inches of black bituminous shale filled with encrinites; the second limestone is also four feet in thickness; its colour is dark iron-grey, and obscure traces of organic remains may be seen in it. To this succeeds a thin layer of shale, then three feet of reddish micaceous sandstone, and 10 feet of exceedingly fine-grained white micaceous sandstone. In this soft rock the coves, three in number, are excavated; their floor is of red and white laminated micaceous sandstone, over which the tide flows at high water. At the extremity of the rocks at Snipe Point, which forms the western side of the haven at the coves, the undulation of the strata may be seen to the greatest advantage, and might be compared to the waves of the sea, but their curves are too regular, passing across the inclination of the beds at right angles, which is to the south-east. This limestone comprises 12 distinct strata, measuring in all 16 feet; the whole of these are exposed to view at low water, having been broken across by the violence of the ocean. Its position is below the sandstone at the coves, and above a red and white sandstone in the outer part of the haven to be seen only when the tide is quite low. The limestone first makes its appearance on the beach north of Snipe Point, and is again lost near Emanuel Head. Between its first and second strata, which are each a foot thick, is enclosed a bed of shale of the same thickness, containing *mineral charcoal,* but I was never able to detect vegetable impressions in the shales of this island, though casts of euphorbiæ are not rare in the sandstones. The colour of the limestone is smoke-grey, and bivalves and encrinites are dispersed through it. From the outer part of the reef on the north side of the haven at the coves,* a dyke crosses the strata, and passing through the rocks below the southern point may be again observed on the beach beyond it. The chasm is six feet wide, and filled with limestone in distinct concretions, the colours of which pass from dark reddish-brown to greenish-white, mixed with small veins and minute crystals of white calcareous spar in druses.

That sand hills cover the Snook I have already mentioned: from thence to the neighbourhood of the town, the shore is low, and gradually declines into Fenham Flats without rocks protruding from below the soil; but at a short distance within the line of sand, an extensive quarry has been worked in fine-grained white micaceous sandstone.

Approaching the town, a cliff of shale rises gradually from the north, till its perpendicular face measures about 30 feet, of which 8 or 10 are diluvium: this bank terminates close to the Heugh. The shale is bituminous, and, from exposure to the atmosphere, is fragile, and of a reddish-brown colour. Two bands of clay ironstone, each four inches thick, traverse it horizontally, and nodules of the same ore, enclosing septariæ and such plates as the pitmen call girdles, together with cubic pyrites, are scattered through the whole rock. From the same shale at the foot of the cliff, fragments of the encrinal fossil, formerly highly prized under the name of St. Cuthbert's beads, occur in abundance. Of a shaft that was sunk near this spot, I could obtain no further information than the seam of coal penetrated to, being only 14 inches in thickness, was not worth working, though fuel is a great desideratum both for house use, and for burning lime. It is either imported from Newcastle, and subject to a duty, or is brought in small carts from the vicinity of Berwick. Having finished the survey of the coast, little remains to be added, except that tradition points a low field between the town and the basin, as the spot from whence the stone is said to have been quarried for the erection of the abbey; it is chiefly of a dirty brick-red colour with small spangles of mica, and though fine-grained and soft, has resisted the action of the elements remarkably well. The millstone grit does not appear *in situ*, though it creeps out on the main both to the north and south of the island. From good authority I learn, that glass tubes similar in composition, but smaller in size, to those found at Drig, in Cumberland, have been detected in sands on the shores.

Without woods, moorlands, or rivulets, Lindisfarn of course possesses a scanty Flora; yet from its slender store, a few plants may be selected worthy the notice of botanists unaccustomed to examine such as are indigenous on our sea shores. Cryptogamic species are peculiarly scarce, with the exception of marine algæ, but these being common on all the northern coasts, and already mentioned in the Botanist's Guide through Northumberland and Durham, need not be recapitulated.

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* This dyke was first noticed by Mr. Culley.

Plants on the Heugh and Castle Rock.

Poa distans,
Aira cristata,
Allium oleraceum,
Silene maritima,
Statice armeria,
Carduus marianus,
Trifolium scabrum,
Trifolium striatum,
Pyrethrum maritimum,
Plantago Coronopus,
Parmelia olivacea,
Parmelia parella.

On the Links. North Side of the Island.

Schœnus compressus,
Parnassia palustris,
Samolus Valerandi,
Erythræa littoralis; chironia littoralis. Eng. Bot. t. 2305.
Geranium sanguineum,
Erodium cicutarium γ Fl. Brit.
Anagallis arvensis,
Erigeron acre,
Bœomyces alcicornis.

On the Sea Shore.

Plantago maritima,
Salicornia herbacea,
Aster Tripolium,
Cochlearia anglica,
Cochlearia officinalis,
Triglochin maritimum,
Hordeum maritimum,
Bunias Cakile,
Zostera marina on Fenham Flats.

On St. Cuthbert's Isle.

Statice Limonium. Its northern limit on the east coast.
Parmelia scopulorum.

In the Loch or on its Shore.

Littorella lacustris, } Miss Emma Trevelyan.*
Alisma ranunculoides, }
Spergula nodosa,
Triglochin palustre,
Ranunculus Flammula δ reptans. Fl. Brit.

On the Abbey.

Cheiranthus fruticulosus.

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* To this accomplished botanist, the Flora of Northumberland is indebted for the discovery of Linnæa borealis, growing together with Trientalis europæa and Pyrola minor var. rosea, in a fir plantation on the edge of the moors at Catcherside, four miles west of Wallington, Nuphar minima of Eng. Bot. Nuphar Kalmiana of Hooker's Flora Scot. in Chartner's Lough. On the moors in the same vicinity, Pyrola media near Roadley Lake, and Gyrophora pustulata, in abundance. On the millstone grit rocks called Shaftoe Crags, Bolton mentions the neighbourhood of Halifax as a [illegible] of this lichen, and the Rev. John Harriman gathered it near Irton Hall in Cumberland, *but to* Northumberland, it is new; nor has it been found in Durham.

In the Fields and Pastures.

Convolvulus arvensis, rare;
Carduus arvensis flore albo, common;
Delphinium consolida; field near the Lough House, rare;
Salix mollissima. In hedges.
Agaricus campestris,
Agaricus Georgii, } abundè.
Agaricus orcades,
Agaricus aurantius.

Through the kindness of a young lady who frequently visits Lindisfarn, I am enabled to subjoin the following list of shells. It was taken from a collection made by her during the summer months, when good specimens can be procured from the fishermen's lines, such as are cast on shore being generally broken and spoiled. By the enumeration, the conchologist will be enabled to form a tolerably correct idea of the species afforded by this sea. The names are those used in Dillwyn's Descriptive Catalogue.

Chiton marginatus,
Lepas Balanus,
L. balanoides,
L. anserifera,
L. anatifera,
Pholas Dactylus,
P. crispata,
Mya arenaria,
M. declivis,
Solen Siliqua,
S. Legumen,
Tellina ferroensis,
T. fabula,
T. balaustina,
T. crassa,
T. carnaria,
T. Zonata,
T. cornea, in the Loch,
Cardium echinatum,
C. edule,
C. rubrum; W. C. Trevelyan, Esq.
Mactra stultorum,
M. solida,
M. truncata,
M. subtruncata; W. C. Trevelyan, Esq.
Mactra piperata,
M. Boysii; W. C. Trevelyan, Esq.
M. lutraria,
Donax truncatulus,
Venus fasciata,
V. casina,
V. scotica,
V. islandica,
V. spuria,
V. exoleta,
V. decussata,
V. perforans,
V. virginea,
Arca Nucleus,
Ostrea maxima,
O. varia,
O. sinuosa,
O. obsoleta,
O. opercularis,
O. edulis,
Anomia Ephippium,
Mytilus rugosus,
M. edulis,
M. incurvatus,
M. modiolus,
M. anatinus, in the Lough,

Cypræa Europæa,
Bulla aperta,
B. fontinalis. In the Lough.
B. flexilis. Wern. Trans.
Buccinum lapillus,
B. undatum,
B. macula,
Strombus pes, pelecani,
Murex antiquus,
M. corneus,
Trochus cinerarius,
T. zizyphinus,
Turbo litteratus,
T. terebra,
Scalaria Trevelyana. Leach MS.
Helix rufuscens,
H. crenulata,
Helix complanata. In the Lough.
H. nemoralis,
H. grisea,
H. stagnalis,
H. fossaria, } In the Lough.
H. putris,
Nerita canrena,
N. glaucina,
N. littoralis,
Patella vulgata,
P. pellucida,
P. lævis,
P. fissura,
P. græca,
Dentalium entalis,
Serpula spirorbis.

Pennant has given an account of the birds which breed on the Farn Islands, and of course are to be met with on these shores. However, it may be worth mentioning, that this is the most southern spot where the eider duck is known to rear its young. The small Lough on Lindisfarn is the occasional resort of wild swans, geese, widgeons, seal, &c. The wild duck is here a native, and the domesticated sheldrake may be seen in company with the tame ducks. The larger seal, phoca barbata, inhabits the rocks of the staples and farns; and the lesser seal, phoca vitulina, the shoals of Lindisfarn.

I shall conclude by observing, *that strangers visiting Holy Island, should not attempt to pass over Fenham Flats till the tide has ebbed between two and three hours, or when it is within the same space of time of high water, especially if the weather be foggy.*

I am, Sir, yours truly,
N. J. Winch.

Article IV.

On the Greek Fire. By the Rev. J. J. Conybeare, MGS.

(To the Editor of the *Annals of Philosophy.*)

MY DEAR SIR, *Bath Easton, Nov. 5, 1822.*

In your number for November, you have inserted the substance of a very ingenious essay by Dr. Macculloch on the Greek Fire. I venture, therefore, to transmit the following remarks in the hope that they may contribute somewhat more to the illustration of this curious subject, and to the amusement of your chemical and antiquarian readers. They will be found to contain some notices which (so far as my reading extends) have escaped the observation of former inquirers, and I have endeavoured to render them as concise as possible.

Fully agreeing with Dr. Macculloch that more than one substance may have been used and described under this name, and that in all probability we are ultimately indebted to the east for the knowledge both of these compounds, and of gunpowder, I would still venture to suggest on the latter point that our acquaintance with Indian and Chinese literature (great and creditable as it is to our learned countrymen) has not yet made such advances as to entitle us to quote even with tolerable confidence, documents, in the languages of those regions, pretending to remote antiquity. The critical tests which have been so rigorously and successfully exercised on the classical remains of Greece and Rome, have been as yet but sparingly applied to the examination of the Sanscrit and the Chinese. Our orientalists, like the scholars of the fifteenth century, have been employed in the more important task of mastering the difficult and obsolete dialects of their new empire; in searching out, collecting, and making public, the materials for future criticism; but at present we can scarcely hope to separate with any precision that which is fictitious or interpolated from that which is genuine and uncorrupted; and the almost uniform tone of oriental literature is such as in truth to induce all sober inquirers to lean much to the side of caution. There is also, as Dr. M. observes, a fabulous air about the Indian story related by Philostratus (a writer in no case of very high authority). I would suggest too that it bears every appearance of being a direct imitation of the more classical tale which records the protection twice afforded to the sanctuary of Delphi by its tutelary god, first against the Persian, and in later times against the Celtic invaders. Herodotus and Diodorus Siculus relate the former, and Pausanias the latter. If these accounts be not altogether fabulous, it seems probable that the sacred College of Delphi possessed the secret of fabricating some powerfully explosive compound. The Grecian Camden describes the continued thunders and lightnings, destroying not, as usual, single individuals only, but burning and injuring all who stood within reach. These were accompanied by repeated shocks of earthquakes (earthquakes, it will be recollected, were also among the prodigies of the Eleusinian mysteries). Immense masses of rock were launched, he tells us, upon the aggressors, wherever collected in any numbers, *as at a mark*, "*σκοπον τους βαρβαρους ειχον.*" As this took place during the night, it might indeed have been done by mere mechanical force, the Greeks profiting by the cover of darkness; but a warm fancy might, from the words quoted, conclude at once that, if not artillery, some means were used which enabled the defendants *to take aim.* One regrets that Apollo had not reserved a portion of his bolts to avenge the later depredations of Nero.

But to leave the regions of conjecture, I am indebted to that strange mixture of learning and absurdity, the Magia Naturalis of Baptista Porta for reference to an earlier authority on the subject of the Greek fire (or of a compound at least answering closely to its description) than any of those produced by Beckman, Dutens, or Dr. M. It seems indeed to have escaped the notice of Gibbon himself, who must nevertheless have read it. Ammianus Marcellinus, in detailing the immense preparations of Julian for his last campaign, particularizes among the warlike engines one which was named Malleolus. He describes it as a dart having between its shaft and point a species of iron cradle with many apertures. The interior of this was filled with an inflammable compound (ignem cum aliquo alimento). It was to be thrown from a weak or slackened bow (arcu invalido aut remissiore), as it was liable to be extinguished (perhaps, before it was fully ignited) by passing rapidly through the air. Wherever it fell, it burned *tenaciter;* water served only to increase the vigour of the flame which was extinguishable by dust alone (nec remedio ullo quam superjacto pulvere consopitur). This is precisely the " non enim extinguitur aquâ sed arenâ " of the monkish rhymer. Within a few pages, Ammianus has another passage which seems to establish the identity of one variety of naphtha with the inflammable ingredient which gave its chief energy to the Greek fire. " Hic (in Assyriâ) naphtha gignitur picea, specie glutinosa, similis ipsa quoque bitumini et cum hoc liquoris ardere cœperit genus, nullum invenit humana mens præter pulverem exstinguendi commentum." He soon afterwards describes the oleum Medicum as used in the same manner with the charge of the malleolus (he nearly repeats indeed his former words), and states it to be prepared by mixing common oil with a species of herb, and, after long digestion, thickening it yet more by the addition of a species of naphtha. " Oleum usûs communis herbâ quâdam infectum condiunt harum rerum periti, ad diuturnitatem servantes, et coalescens durant ex materiâ venæ naturalis similis oleo crassiori, quæ species gignitur apud Persas quam ut diximus Naphtham vocabulo appellavere gentili." Thus a composition answering in its use and effects to the Greek, or as it is termed by Theophanes, the Roman fire, appears to have been well known at least 300 years before its supposed invention by Callinicus. He may indeed have revived its use, or improved its composition.* In fact, Pliny, at a yet earlier period, describes the maltha nearly in the same manner, and as employed for the same purposes. "Cum quid adtigit solidi adhæret præterea tactus sequitur fugientes. Sic defendêre (Comagenes incolæ) muros oppugnante Lucullo, flagrabatque miles armis suis aquis etiam accenditur terrâ tantum restingui docuere experimenta."* In the age then of Lucullus, we have the use of this compound, (or at least of its most energetic constituent) restricted to an eastern people,—a strong corroboration of the conclusion at which Dr. M. has arrived from other premises. But there is a yet earlier though less respectable testimony to the existence of a like oleum incendiarium to be found in the remains of the marvel-monger Ctesias. He affirms that the mountain chimæra sends forth constant flames, which are increased by water, but extinguished by earth.† Ælian has preserved a still more curious version of the properties and use of naphtha from the same Ctesias. He relates that a gigantic worm is found in the Indus, from whose body is obtained an oil capable of burning any thing with which it comes in contact, even without the application of fire. With this, it is said, he adds, that the Persian monarch besieges and subdues towns, needing and using no other engine. He has merely to throw an earthen vessel filled with the destructive fluid within the walls, or against the gates, and resistance becomes useless. It can be extinguished only by heaping on it earth and rubbish.‡ Photius has an extract from the same quarter to the same purpose.§ Strabo also mentions both the solid and liquid varieties of naphtha. He states that it *may* be extinguished by a *very large quantity of water*; but that it may be quenched by dust, alum, vinegar, or birdlime‖ (ιξῷ). He alleges the authority of Eratosthenes. It would probably be no difficult task to multiply yet further our references to early authorities,¶ but enough has been, I think, adduced to show that the Greek fire was known to the Romans before the time of Callinicus, or even of Constantine.

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* It may be observed that the Malleolus is mentioned by Livy and other writers anterior to the age of A. Marcellinus (V. Forcellini Lex. in voce); but as they appear to be silent with respect to the composition of its *charge*, I forbear to quote them.

I pass to the paragraph quoted from the Speculum Regale. This is, as Dr. M. justly remarks, very obscure. To me it bears the appearance of an extract from some Scaldic poem; at least, it is conceived in the metaphorical style of their versification. I should decidedly prefer reading with the MSS. elldligum for eiturligum loga (q. d. ignes flammeos). Skialldar Jautun would poetically be used to express the gigantic destroyer of shields, or even of fortifications (hostis giganteus testudinum): the incurvus (biugur) may refer either to a large cross bow or to the spring of the balista. This, it may be said, is a forced interpretation, but many of their well-known metaphors have an aspect far more harsh and enigmatical.

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* Plin. Hist. Nat. l. 2, c. 104.

† V. Ctesiam in app. Herodot. Wesseling. p. 860. V. et. Plin. Hist. Nat. l. 2, c. 106.

‡ Ctes. ut supra, p. 864.

§ Ctes. p. 832.

‖ Strabo. Ed. Oxon. p. 1055.

¶ I have not had the opportunity of consulting Vegetius, the Poliorceticon of Lipsius, the works of Arrian, or of Quintus Curtius, or the Glossary of Ducange. My editions *of Pliny and Ammianus Marcellinus* are unfortunately without notes.

On the subject of the Greek fire mentioned by Joinville, I regret that I cannot see quite so clearly as Dr. M. does, the proof of its resemblance to any thing in our modern artillery. The term petrarium or perriere seems to have been applied commonly to that variety of balista which threw large stones. Had it been a mortar, Joinville would, I should think, have mentioned its novelty. There must be some discrepancies too in the MS. text of his description. The only edition within my reach (Paris, 8vo. 1785) does not any where mention the carcase as sent from the bottom of the Perriere. Allowing this, however, to be the correct reading, it would be equally descriptive of that variety of balista in which the missile body was projected from a cup attached to the end of a lever strained backwards until parallel, or nearly so, with the horizon. Joinville, moreover, states, that the fire was extinguished, and that in one case by a single man, "par ung home que avions, propre a ce faire." In fact, the dread of the honest chronicler and his companions was not so much that of personal injury from the fire, as from the destruction of their wooden cuniculi or cat-castles (chatz chasteilz). It would surely be beyond the power of a single man to extinguish a caisse filled with an inflammable compound of which nitre made a part, while a barrel or cradle-full of tow dipped in bituminous matter, if at a distance from any thing else inflammable, might be smothered up with sand and dirt at no great peril.*

In later times, the use of artillery appears, as Dr. M. remarks, gradually to have driven the Greek fire off the stage; but as its use decreased, the recipe for its fabrication became much more complicated and mysterious. V. Biringuccio † gives a most formidable list of the substances required for ensuring the highest degree of success in such compounds. Among these the oleum sulphuris is almost invariably prominent, an addition which must, by decomposing the nitre, have rather lessened than added to their force. The very intricacy and clumsiness of his formulæ show that such mixtures were becoming rapidly the objects rather of quackery than of practical use. At a still more recent period, Fludd, the well-known mystic, declines revealing the composition used for fire-pots, as being a secret which belonged to his country.‡

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* My edition of Joinville contains a long note on the Greek fire by the learned Du Fresne: he carries it no higher than Callinicus. He quotes two remarkable passages from the Tactics of Leon and the Alexias of A. Comnena; but there is some obscurity in his interpretations, especially of the latter; and I have not at present access to the original text of either.

† Pirotechnia, l. 10, c. 9. The same writer gives (l. 10, c. 5) a curious description of a squib or rocket, made of wood or iron, and capable of throwing stones or balls. *This, which has been transcribed into later works on pyrotechny, or some like rude attempt, may have* suggested the rockets now in use.

‡ *Fludd Macrocosmus*, p. 422.

I fear that the best apology which can be offered for the length of this memorandum will be found in its date. It is at least *seasonable*.

Believe me, my dear Sir, very truly yours,

J. J. Conybeare.

In looking over Ctesias, I found a curious anticipation of the use of conductors for lightning which I do not recollect to have seen noticed. He relates that a certain variety of iron is found in India, which, when fixed into the ground (πηγνυμενος εν τη γη), has the power of averting storms and lightnings (πρηστηρας).*

I would take this opportunity of correcting an apparent inaccuracy in my account of V. Biringuccio. He is the first writer with whom I am acquainted who mentions manganese *by its present name.* Earlier writers (as quoted by Beckman) allude to its use, but term it magnes, or magnesia. I might have added, that Biringuccio is mentioned with respect by Du Fresne and Beckman. Allow me to apologize for a few errors of the press which have crept into that article (originating, I fear, in the indistinctness of my own handwriting): the Italian scholar will readily discover and correct them.—J. J. C.

Article V.

On an Electrical Phenomenon. By M. P. Moyle, Esq.

(To the Editor of the *Annals of Philosophy.*)

Sir, *Helston, Nov. 6, 1822.*

The following circumstance being new to me, and finding no mention made of it by chemical writers, induces me to present it to your readers, some of whom it may possibly interest.

On constructing a thermometer after the usual manner, with a glass tube having an *oval* base, and after it had been hermetically sealed, I found, on the subsidence of the mercury, that the tube was not perfectly free from damp, so that some of the mercury adhered to its sides, and prevented its regular fall. It is necessary in this case to subject the tube to the heat of a spirit-lamp to expel it; this I did without admitting the air; and when the glass was heated, I forced the mercury from the bulb with the heat of my hand past the heated spot: the heat was great, though not *visible*, and as the mercury rose in a gaseous state, and passed to the upper end of the tube where it was condensed, I was struck with the phenomenon of a vivid flash of light (particularly observable in the dark) of a bright blue colour. This continued so long as the heat was sufficient to raise the mercury to a gaseous state.

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* *Ctes.* Indica, ut supra, sub initio.

This light much resembled the electrical spiral tube, and a frequent repetition of the experiment was uniformly attended with similar results. I now admitted the air, and the same effects could not be produced. On using a tube of larger dimensions, and under similar circumstances, the effect was much more striking.

I at first suspected that possibly the mercury might be contaminated with a small portion of zinc, as the brilliant light so much resembled that metal in a state of ignition; but on repeating the experiment with mercury in which no trace of any adventitious ingredient could be discovered, the effects were precisely as before. I am, Sir, your obedient servant,

M. P. Moyle.

Article VI.

On the Temperature of Mines. Communicated to the Cornwall Geological Society, by R. W. Fox, Esq.

The high temperature which prevails in mines having excited some attention, I am induced to submit to the Cornwall Geological Society the result of further observations which have been made on the subject in several mines, since my last communication.

At South Huel Towan copper mine, in the parish of St. Agnes, the temperature of the water in the cistern at the "*sump*," or bottom of the mine (45 fathoms deep), was 60°. This may be taken, therefore, as the mean temperature of the streams of water, which flow through the deepest levels, or galleries, into the cistern. Two men were employed at one time (that is, 8 in 24 hours) in this part of the mine.

East Liscomb, a copper mine in Devonshire, depth 82 fathoms. Temperature of water in the cistern 64°.

Huel Unity Wood, a tin and copper mine in Gwennap Parish, depth 86 fathoms. Temperature of water, taken as before, 64°. Four men constantly worked at the bottom of this mine.

Beer Alston, a lead mine in Devonshire, 120 fathoms deep; water 66·5° of temperature, taken as before.

Poldice, a tin and copper mine, in the parish of Gwennap. Temperature of the water 78° in the lowest cistern, in one shaft, which was 144 fathoms deep. Eight men were constantly employed at a time, at the bottom of this part of the mine, besides two men, during the day (*"on tribute"*). The temperature of the water in another shaft of the same depth, and tried in the same way, was 80°. Two men only were employed at a time in the levels at the bottom.

Consolidated copper mines in Gwennap. One shaft is 150 fathoms deep, and the temperature of the water 76°. Six men were employed at a time at the bottom. The temperature of the water, ascertained in the same way, in another shaft, of the same depth, was 80°; and here there were eight men at work at a time.

Huel Friendship, a copper mine in Devonshire. Temperature of the water taken as above was 64·5° at the depth of 170 fathoms. The number of men employed at the bottom has not been reported; but as they were sinking the engine shaft, there could not be less than two. There is, when its depth is considered, a very small quantity of water flowing into the bottom of this mine; for it requires only a six inch box, and five strokes of the engine a minute, to draw it up. The mine is situated on very elevated ground, bordering the granite hills of Dartmoor.

Although the temperature of the water is probably more than 14° above the mean of the climate in which it is situated, it is certainly much inferior to the temperature generally observed in mines of the same depth.

The undermentioned mines, being partly filled with water, I give the temperature of the water remaining in each.

North Huel Virgin, a copper mine in St. Agnes parish. The temperature of the water, which stood at 39 fathoms under the surface, was 60°.

Nangiles, a copper mine in the parish of Kea. The temperature of the water at 59 fathoms under the surface was 58°. Nangiles is 88 fathoms deep at the engine shaft. The machinery for pumping the water out of this mine had very recently been set to work, and had consequently made but little progress in draining it. I mention this in connection with my remarks on the temperature of stopped mines, in order to account for its not being greater. The veins in this mine are large, and remarkable for the quantity of iron pyrites they contain.

Tresavean, a copper mine in Gwennap. The temperature of the water standing at 100 fathoms under the surface is 60°, and the whole depth of the mine is 170 fathoms. It is situated on elevated ground about 480 feet above the level of the sea, and is moreover in granite, in which the temperature generally appears to be inferior to what is observed in "killas," or clay slate, at equal depths.

Huel Maid copper mine. The water which it contains is 126 fathoms from the surface, 30 fathoms in depth, and 60° of temperature. There are no pumps in this mine, but the water has recently been considerably reduced, in consequence of the reworking and draining of some neighbouring mines; all the water from the higher levels, &c. must, therefore, be mixed with that in the mine, and reduce its temperature, which is in a considerable degree prevented in mines, which are furnished with pumps, by placing cisterns to receive the water at different levels.

Mines which contain much water, if the workings have been only recently renewed, are generally of an inferior temperature to those of equal depth which are drained to the bottom.

This remark applies, in a much greater degree, to mines which have been long stopped, and filled with water; in confirmation of which, the three following instances may be given.

The water in Herland copper mine, in the parish of Gwinear, in the shaft, at the adit level, 31 fathoms deep, is only 54°, though the mine is 161 fathoms in depth.

At South Huel Ann, in the same parish, the water in the shaft was likewise 54°, the depth of the adit being 11, and that of the mine 23 fathoms.

At Gunnis Lake copper mine, in the parish of Calstock, which is 125 fathoms deep, the water in the shaft, at the adit level, 35 fathoms deep, was 57°.

The water that flows out through the adits of stopped mines, is, I presume, derived from the superincumbent strata, or indirectly, by displacing the water in the shafts, or in the upper levels that communicate with them, and which must be, in a greater or less degree, more accessible, and offer an easier outlet to the water than those which are deeper and more remote.

If this be admitted, it follows that the water which issues out of the tops of shafts of stopped mines does not proceed from the *deeper* levels; but, on the contrary, it seems highly probable, that the water they contain is nearly stationary, and as it does not readily communicate heat in a lateral direction, that its temperature may materially vary from that in the shafts; whereas it is well known that in a perpendicular or oblique column of water, an interchange will take place between the warmer part of the liquid column at the bottom, and the colder at the top, till an equality of temperature is produced through the whole.

I attribute the higher temperature of the water in Gunnis Lake shaft, at least in part, to the very elevated ground in its immediate neighbourhood; although the relative temperature of the water in the shafts of stopped mines may also depend, on the greater or less depth, at which the columns of water commencing above the adit level, communicate with the shafts, or with the levels connected with them.

When the working of Tincroft tin and copper mine, in Camborn parish, was recently resumed, after it had been for several months suspended; an opportunity occurred for ascertaining the temperature of the water, when it was sunk to the depth of 126 fathoms under the surface, and was only 10 fathoms deep, in the bottom of the mine. It was then found to be 63°, and this was before many men had resumed their labours, or indeed any of them, at the inferior levels; and moreover, at the time of making the observations, even the few men who worked in the mine had not been in it for the space of nearly two days.

Near the middle of 1819, when the water stood at the same place in the mine, and it was, and had long been, in a state of full working; the temperature of the water at the bottom was only 59°. Perhaps the water will again be reduced to this temperature, if it should remain at the same depth in the mine; for is not reasonable to suppose, that the droppings of colder water down through the shafts must affect the temperature of that at the bottom?

In consequence of an accident in the steam-engine at Ting Tang, the water rose considerably in the mine. On its being again sunk to within 10 fathoms of the bottom, the mine being 117 fathoms deep, its temperature at this station was found to be 63·5°; whereas the water pumped up from the bottom into a cistern immediately above the place of observation was 65°; so that the water seems to have been 1·5° warmer, at the depth of 10 fathoms, than at its surface. This phenomenon must, I think, be attributed to the under current from the level caused by the action of the pumps.

A fact, communicated to me by a gentleman in the brewhouse of Barclay and Co. at Southwark, may here be noticed. Not long ago, a well was sunk, in order to procure water for the supply of the brewery. They did not attain their object, until they had got down 140 feet under the surface, and cut through the great bed of clay, which lies under the metropolis. The water then rose rapidly in the well, its temperature being 54°, which it invariably maintains at all seasons of the year. Now the climate of London and its vicinity is at the mean temperature of 49·5° on the authority of Luke Howard, which is 4·5° under that of the water in the well.

I stated at the last annual meeting of this Society that a thermometer buried at the depth of three feet in a rock in a level at Dolcoath Mine, 230 fathoms under the surface, indicated, during eight months, a temperature of about 75° to 75·5°, when the mine was clear of water. It has subsequently remained in the same place nearly twelve months longer, and the mercury has continued stationary at 75·5°, notwithstanding the changes of the season.

Although, I think, it will be admitted, that the bottoms of our mines are, for many reasons, less liable to be influenced by adventitious causes than the superior levels, I shall give the results of various observations made on the temperature of water in the undermentioned mines, at different levels; and, as far as it was ascertained, of the air also at the same stations; in order to show the relative temperature of both, and the ratio in which it increased in depth, without particularising the mines in which the experiments were respectively made, as this appears to be unnecessary.

The mines referred to were South Huel Towan, East Liscomb, Huel Unity Wood, Beer Alston, Poldice, the Consolidated Minas, Huel Friendship, the United Mines, Treskerby, Huel Damsel, Ting Tang, and likewise Huel Maid, Nangiles, North Huel Virgin, and Tresavean. The four last mentioned mines having been partly full of water for many years, the figures which refer to them are distinguished by an asterisk.

Depth from the surface in feet.	Depth from the surface in fathoms.	Medium in which the observations were made.	Degrees of temperature.							
60	10	Water.								
		Air.								
120	20	Water.	58°							
		Air.	56							
180	30	Water.	55	64						
		Air.	56	58						
240	40	Water.	56	60	54	*54	*56	*56	*60	
		Air.	57	60	60	*55	*57		*60	
300	50	Water.	60	60	60	60				
		Air.			58	58				
360	60	Water.	60	62	60	58	*58			
		Air.	61	62		60	*57			
420	70	Water.	61	*58	*58					
		Air.	61		*59					
480	80	Water.	64	64	62	*59				
		Air.	65	64	64					
540	90	Water.	64	66						
		Air.	62	67						
600	100	Water.	65	*60	*60					
		Air.	66		*56					
660	110	Water.	64	66	64					
		Air.	65	66						
720	120	Water.	66½	66						
		Air.	68	68						
780	130	Water.	66	72	74	74	63	72	68	*60
		Air.		73	74		62	73	70	*58
840	140	Water.	78	70	80	72				
		Air.	78	72	81	75				
900	150	Water.	76	80						
		Air.	72	80						
960	160	Water.	66							
		Air.	73							
1020	170	Water.	64½	77	84					
		Air.	66	76						
1080	180	Water.	72	69	86	87				
		Air.	74	72	88					

In taking the temperature of the water in the different levels of mines, care was generally observed to select the largest streams, and to put the thermometer at or near the places where they first flow into the mines, so that the influence of any heat from the mines seems to be put out of the question.

It appears that in almost all the mines which have been examined, the highest temperature has been found at the bottom, and it is deserving of notice, that here, in most instances that I have investigated since my last paper, very few workmen are employed; and generally their number increases at each level in ascending from the bottom, as high up as one quarter, or even one-third of the way; so that not very far from the middle of mines, they are frequently the most numerous.

At a level 180 fathoms under the surface in the United Mines, I find the temperature of the water which was, and had been during 12 months, 30 fathoms deep in the mine, was 80°, and a stream of water flowing into the same level, was 87°. This is only half a degree less than it was at the same place in 1820. At that time about 400 men were employed in the mine eight hours each day, and about 50 on an average for the remainder of the 24 hours. When the last observation was made, only about 200 men worked in the mine eight hours a day, and about 50 during the remaining 16 hours.

I do not dispute, that in close levels, where there is no current, the presence of the men increases the temperature of the air, yet it does not appear by the above table that the heat of the air is usually much greater than that of the water in the same places, perhaps on an average not exceeding 1° or 2°. In many instances, indeed, the water was from 1° to 4° warmer than the surrounding air, and this occurred in several mines at or near the deepest levels.

Before I conclude my enumeration of facts, it may, perhaps, be desirable to state the temperature of the water which flows through the great adit, and is discharged near Nangiles mine, above Carnon Valley. This adit traverses the principal mining district of Cornwall, and extends nearly 30 miles, including its different ramifications, and more than five miles from one extremity to the other in one direction, and three miles in another.

The temperature of the water was taken near the mouth of the adit about six weeks since, and was found to be 69·25°. Richard Thomas, land-surveyor of Falmouth (author of an interesting map of a large portion of our mining district), has ascertained, by frequent observations, that the quantity of water discharged by the adit at different times of the year, has varied from 910 to 1644 cubic feet per minute; but as some deep mines have been set to work since he made his experiments, the average quantity is now probably greater. It appears, on making a comparison of the depth of the water at the time the foregoing temperature was ascertained, with his calculations, that the quantity discharged was equal to 1400 cubic feet per minute, or about 60,000 tons per day.

The great adit is divided into three principal branches, the first of which unites with it, at about a mile from its mouth, and communicates with the United and the Consolidated Mines, Huel Squire, Ting Tang, Huel Maid, and South Huel Jewel, the average depth of which mines seems to be about 150 to 160 fathoms. The temperature of the water in this branch near the junction, and about one mile and a half from the mines which principally supply it with water was 73·5°, about the end of last month, when this and the following observations were made. At nearly a mile further on, the great adit is divided into two branches; one of them receives the water from Poldice, Huel Unity, Huel Unity Wood, Huel Damsel, Huel Purk, Rose Lobby, Huel Hope, Huel Gorland, Huel Jewell, and Huel Clinton; the average depth of which is, perhaps, from 110 to 120 fathoms; and the temperature of the water in the branch, at about a mile from the principal mines above named, was 66·5°.

The other branch is connected with Treskerby, Huel Chancery, Chacewater, North Downs, Creegbraws, Huel Boys, Cardrew, and a few smaller mines; their average depth may be estimated at 100 to 110 fathoms, and the temperature of the water in the adit, about three miles and a half from the mines, was 65°.

I have not ascertained the quantity of water discharged by each of these branches; but it is evident they carry off not only the water pumped from the various levels of the respective mines, but also that which is drained from the strata under which they pass, and which is from 30 to 50, and in some places from 60 to 70 fathoms in thickness.

The temperature of the water in the adit is, therefore, even more considerable than might be expected, and the difference observed in the branches may be attributed to the relative depths of the mines with which they are connected, and to many of those communicating with the two last mentioned branches being stopped, or partly full of water.

I have mentioned that the water flows into cisterns at different levels in mines, being partly or entirely retained by the rock on which it rests, but generally from the strata being more or less porous, some of the water sinks through it, and may either mix with an inferior portion, before it flows into the levels, or it sometimes descends in numerous drops, or small streamlets, from the roofs of deeper levels; and in either case, it must produce more or less influence on the temperature, and prevent its being uniform at equal depths.

If there were a perfectly free and open communication between the various portions of water under the surface of the earth, it is evident that mines could not be drained, but the pressure of the columns of water would be irresistible, and their impetuosity overwhelming.

The high temperature in mines seems to have no necessary connexion with the minerals which they contain, since where iron pyrites is very abundant, the heat does not appear to be greater than where it is the reverse.

If, as we may conclude from the evidence adduced from various quarters, that the high temperature which exists under the surface of the earth does not arise from causes merely local or accidental, must we not suppose it either to have been imparted to the globe at its creation, or attribute it to some cause constantly in operation? If the former hypothesis be adopted, it cannot readily be conceived that the heat is conducted towards the circumference of the earth, by the solid substances of which it is composed; for if so, the internal heat must be intensely, and indeed incredibly great; besides, many facts oppose this conclusion; among which it may be proper to notice that granite, and other hard rocks, are generally of rather an inferior temperature compared with clay-slate, and other more porous and softer rocks, which are worse conductors of caloric. It is true we may imagine water and vapour to convey and diffuse heat from the interior of the globe towards the surface, and not necessarily adopt the conclusion, that the heat must be so intense at the centre; but, without setting aside the agency of water and vapour in circulating and equalizing the temperature, may it not with more probability be supposed to depend upon some constantly operating cause? If electricity, for instance, be evolved when several different mineral substances are brought into contact, and likewise in the process of crystallization, &c. may it not, in connexion with the strata and veins, and the almost distinct portions of water which abound in the earth, also act its part on a larger scale, and not only excite heat, but contribute to produce the extraordinary aggregation and position of homogeneous minerals in veins, &c. and the beautiful order which exists even under the surface of the earth? I venture to bring this forward merely as a suggestion, hoping, if it be thought to deserve any attention, that others more competent than myself will investigate the subject.

Note.—I will here mention a fact which I consider to be connected with electricity. Having fastened a piece of iron pyrites with a brass wire in a moss house, the moss being damp, I found on the following day that the wire was broken, and excessively brittle, and the parts which had touched the pyrites were much corroded. On one occasion, after the brass wire had been fastened once or twice round a piece of iron pyrites, and had remained for some days enveloped in damp linen, the constituents of the brass wire were separated, and it was converted into copper wire coated with zinc.

Having recently tried some experiments on the water taken from the bottom of several deep mines, I find it, in most instances, to contain in solution a very minute quantity of any foreign substance, varying, perhaps, from one to five or six grains in a pint. Its relative purity appears to have no reference to the depth or temperature of the mines; for instance, Huel Abraham, and Dolcoath, are the two deepest, and two of the warmest mines in the county; and the water from the bottom of these mines does not, in either case, hold in solution more than about two grains of foreign matter in a pint. On the other hand, some mines abound with much less pure water. That from the Consolidated Mines leaves 10 grains of residuum from a pint; Huel Unity, 16 grains; from one shaft in Poldice, 19; and from another 92 grains, from the same quantity. In most of the mine-water that I have examined, the muriatic salts, especially the muriates of lime and of iron, are most abundant. I have detected *muriate of soda* in some instances, particularly in the water from the bottom of the United Mines, the Consolidated Mines, Huel Unity, and Poldice.

Out of the 92 grains of residuum produced from a pint of water from one of the engine shafts of the latter mine, 24 grains proved to be the *muriate of soda*, 52 grains the muriates of lime and magnesia, chiefly the former, and the remainder muriate of iron, and a small quantity of the sulphate of lime.

The water from another engine shaft of the same mine contained 5½ grains of muriate of soda, and about 13 grains of the muriates of lime and magnesia, and the carbonated oxide of iron. All the mines above enumerated are situated in the interior of this part of Cornwall, and are distant several miles from the sea!

Article VII.

On the Depression of the Barometer in Dec. 1821.

By M. P. Moyle, Esq.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Helston, Nov.* 6, 1822.

Perceiving a wish of Prof. Brandes in the *Annals* for Oct. that every particular relative to the great depression of the mercury in the barometer in Dec. 1821, might be minutely detailed as it occurred in England, I beg to give my observations beginning with the 24th of the month.

1821.	Barom.	Mean.	Ther.	Wind.	Strength.	Weather.
Dec. 24.— 8 a. m.	28·465	—	37	E	Brisk.	Fine.
1 p. m.	28·285	28·296	44	E	Brisk.	Cloudy.
10 p. m.	27·960	—	43	N	Very brisk.	Heavy showers.
Dec. 25.— 8 a. m.	28·284	—	39	W	Gentle.	Cloudy.
1 p. m.	28·326	28·316	45	W by N	Brisk.	Very fine.
10 p. m.	28·340	—	39	W	Brisk.	Cloudy.
Dec. 26.— 8 a. m.	28·355	—	39	NW	Strong.	Heavy rain.
1 p. m.	—	28·455	—	NW	Boisterous.	Showers.
10 p. m.	28·555	—	42	NW	Boisterous.	Showers.
Dec. 27.— 8 a. m.	28·683	—	42	NW	Boisterous.	Heavy showers.
1 p. m.	28·787	28·786	46	W	Boisterous.	Showers.
10 p. m.	28·890	—	49	W	Very brisk.	Cloudy.
Dec. 28.— 8 a. m.	27·872	—	45	SE	Boisterous.	Heavy rain.
10 a. m.	27·830	—	—	SE	Boisterous.	Heavy rain.
11 a. m.	27·620	—	50	SW	Boisterous.	Heavy rain.
1 p. m.	27·652	27·738	50	SW	Boisterous.	Showery.
7 p. m.	27·744	—	—	SW	Boisterous.	Showery.
11 p. m.	27·821	—	42	W	Boisterous.	Showery.
Dec. 29.— 1 p. m.	28·742	—	47	NW	Boisterous.	Rain.
Dec. 30.— 1 p. m.	29·220	—	50	NW	Brisk.	Showery.
Dec. 31.— 1 p. m.	30·038	—	49	NW	Brisk.	Fine.

On the 28th, the mercury was at the lowest level with us, though it appears to have been on the 25th on the Continent. The heights taken at 10, 11, aud 1 o'clock, were by measuring the column of mercury from its surface in the reservoir by an accurate rule, and not by the graduated plate affixed to the instrument; but the reservoir is so large that even this great fall made but 0·034 parts of an inch difference in measuring it by the detached scale and the affixed.

I should in the next place state, that my house, or rather the site of the barometer, is 105·30 feet above the level of the sea, for which elevation there ought to be an allowance of 0·104 inch in the height of the column of the mercury. This will reduce the lowest height to 27·516, for 27·620 − 0·104 = 27·516. This deduction should be made from all the results given by me for the true height.

I am, Sir, your obedient servant,

M. P. Moyle.

ARTICLE VIII.

Astronomical Observations, 1822.

By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Oct. 17. Immersion of Jupiter's third satellite	17h	27′	14″	Mean Time at Bushey.
	17	28	35	Mean Time at Greenwich.
Oct. 18. Immersion of Jupiter's first satellite	14	10	27	Mean Time at Bushey.
	14	11	48	Mean Time at Greenwich.
Oct. 26. Immersion of Jupiter's second satellite	7	27	42	Mean Time at Bushey.
	7	29	03	Mean Time at Greenwich.
Nov. 2. Immersion of Jupiter's second satellite	10	05	39	Mean Time at Bushey.
	10	07	00	Mean Time at Greenwich.
Nov. 3. Immersion of Jupiter's first satellite	17	58	02	Mean Time at Bushey.
	17	59	23	Mean Time at Greenwich.
Nov. 5.* Emersion of Jupiter's third satellite	9	15	02	Mean Time at Bushey.
	9	16	23	Mean Time at Greenwich.
Nov. 15. Immersion of Jupiter's third satellite	9	28	07	Mean Time at Bushey.
	9	29	28	Mean Time at Greenwich.
Nov. 16. Immersion of Jupiter's first satellite	8	49	38	Mean Time at Bushey.
	8	50	59	Mean Time at Greenwich.

ARTICLE IX.

On Compound Interest. By Mr. James Adams.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Stonehouse, near Plymouth, Nov. 4, 1822.*

If you can allow the following method of demonstrating the principal theorems of compound interest a place in the *Annals of Philosophy*, I will thank you for its insertion therein.

I am, Sir, your humble servant,

James Adams.

Article 1.—Let P be any principal sum put out at compound interest, to find the amount in n years.

Let $\frac{1}{r}$ be the increase or *interest* of 1*l.* in the first year, then will $1 + \frac{1}{v}$ = *amount* of 1*l*. at the end of the first year. In like manner, $1 + \frac{1}{v} + \frac{1}{v}\left(1 + \frac{1}{v}\right) = \left(1 + \frac{1}{v}\right)^2$ = amount of 1*l*. at the end of the second year.

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* According to the Nautical Almanac, the emersion should have taken place at [illegible], by the Connaissance des Tems at 7h 35′ 15″, but the actual observation occurred at 9h 16′ 23″.

$\left(1 + \frac{1}{v}\right)^2 + \frac{1}{v}\left(1 + \frac{1}{v}\right)^2 = \left(1 + \frac{1}{v}\right)^3$ = amount of 1*l*. at the end of the third year.

$\left(1 + \frac{1}{v}\right)^3 + \frac{1}{v}\left(1 + \frac{1}{v}\right)^3 = \left(1 + \frac{1}{v}\right)^4$ = amount of 1*l*. at the end of the fourth year.

……………………………………………

$\left(1 + \frac{1}{v}\right)^n$ = amount of 1*l*. at the end of the *n*th year. If the last expression be multiplied by the principal P, and $\frac{1}{v} = r$, we shall have $P\ (1 + r)^n = s$ = amount of P in *n* years; from whence P, *r*, *n*, may be found.

Art. 2.—If the payments of interest be supposed to be made at *m* equal intervals in a year, then will the interest of 1*l*. for one interval be $\frac{r}{m}$, and the number of intervals in *n* years *m n*; therefore, by writing $\frac{r}{m}$ for *r*, and *m n* for *n* in the last equation, we shall have $P\left(1 + \frac{r}{m}\right)^{mn} = s$.

When P = 1*l*. and *m* infinitely great, then will $\left(1 + \frac{r}{m}\right)^{mn}$ $= 1 + r\,n + \frac{r^2\,n^2}{1\,.\,2} + \frac{r^3\,n^3}{1\,.\,2\,.\,3} + \frac{r^4\,n^4}{1\,.\,2\,.\,3\,.\,4}$ + &c. = a number corresponding to the common log. *rn* × ·43429448, &c.

Art. 3.—To find the present value of P, due at the end of *n* years.

By continuing the series in Art 1, downwards, we have

$\left(1 + \frac{1}{v}\right)^{-1}$ = amount of 1*l*. 1 year since,

$\left(1 + \frac{1}{v}\right)^{-2}$ = amount of 1*l*. 2 years since,

$\left(1 + \frac{1}{v}\right)^{-3}$ = amount of 1*l*. 3 years since,

$\left(1 + \frac{1}{v}\right)^{-4}$ = amount of 1*l*. 4 years since;

……………………………

$\left(1 + \frac{1}{v}\right)^{-n}$ = amount of 1*l*. *n* years since.

The last expression being multiplied by the principal P; and

$\frac{1}{v} = r$, will give $\frac{P}{(1+r)^n}$ = amount of P n years since, or the present value of P due at the end of n years.

Art. 4.—If the payments of interest be supposed to be made at m equal intervals in a year, then will $\frac{P}{\left(1+\frac{r}{m}\right)^{mn}} = s^1$; see Art. 2. If P = 1*l*, and m infinitely great, then will

$$\frac{1}{\left(1+\frac{r}{m}\right)^{mn}} = 1 - nr + \frac{n^2r^2}{1.2} - \frac{n^3r^3}{1.2.3} + \frac{n^4r^4}{1.2.3.4} + \&c.$$

= a number corresponding to the common log. $- nr \times {\cdot}43429448$, &c.

Art. 5.—If the principal P, instead of increasing as in Art. 1, *decrease* in the same ratio, we shall have, by a like manner of reasoning, $P\left(1 - \frac{1}{v}\right)^n = P(1-r)^n = s$, the decrease of P in n years, and $\frac{P}{(1-r)^n} = s^1$ = present value of P at the end of n years. Hence it appears, that when the principal is continually increased by the *addition* of the interest, the present value of P to be received n years hence will of consequence be *less* than P; so, on the contrary, when the principal is continually diminished by *subtracting* the interest, the present value of P, to be received n years hence, will be *greater* than P.

Art. 6.—If in Art. 1, $m\,P = s$; then will $P(1+r)^n = m\,P$; therefore $(1+r)^n = m$.

Art. 7.—So likewise in Art. 5. If $\frac{P}{m} = s$, then will $P(1-r)^n = \frac{P}{m}$; therefore $(1-r)^n = \frac{1}{m}$.

Art. 8.—To find the *amount* of 1*l.* payable every v years during n years, the interest payable yearly.

Let 1*l.* represent the sum payable every v years.

$(1+r)^v$ = amount at the end of the first period,
$(1+r)^{2v}$ = amount at the end of the second period,
$(1+r)^{3v}$ = amount at the end of the third period,

………………………………

$(1+r)^{n-v}$ = amount at the end of the v periods,

therefore, $1 + (1+r)^v + (1+r)^{2v} + (1+r)^{3v} + \ldots\ldots\ldots (1+r)^{n-v} = \frac{(1+r)^n - 1}{(1+r)^v - 1}$ = amount of 1*l.* payable every v years during n years …………… (*a*)

Art. 9.—When $v = 1$, equation (*a*) will become $\frac{(1+r)^n - 1}{r}$ = amount of an annuity of 1*l.* per annum for n years, and $a \times \frac{(1+r)^n - 1}{r}$ = amount of an annuity a, for n years.

Art. 10.—If the interest be payable m equal times in each year, equation (a) will become $\frac{\left(1+\frac{r}{m}\right)^{mn}-1}{\left(1+\frac{r}{m}\right)^{vm}-1}$ = amount of 1*l*. payable every v years, when the interest is payable m equal times in a year.

Art. 11.—If $v = 1$, and m, as in Art. 10, equation (a) will become $\frac{\left(1+\frac{r}{m}\right)^{mn}-1}{\left(1+\frac{r}{m}\right)^{m}-1}$ = amount of an annuity of 1*l*. per annum, when the interest is payable m equal times in a year.

Art. 12.—If v, instead of representing a multiple of whole years, represent $\frac{1}{u}$th part of a year, then will the expression in Art. 10 become $\frac{1}{u} \times \frac{\left(1+\frac{r}{m}\right)^{mn}-1}{\left(1+\frac{r}{m}\right)^{\frac{m}{u}}-1}$ = amount of an annuity of 1*l*. per annum, payable u equal times in a year, when the interest is payable m equal times, $\frac{1}{u}$ will represent the annuity at first, or the $\frac{1}{u}$th part of the yearly annuity.

Art. 13.—To find the *present value* of 1*l*. payable every v years during n years, the interest payable yearly.

By Art. 3,

$\frac{1}{(1+r)^{v}}$ = present value of 1*l*. at the end of the first period,

$\frac{1}{(1+r)^{2v}}$ = present value of 1*l*. at the end of the second period,

$\frac{1}{(1+r)^{3v}}$ = present value of 1*l*. at the end of the third period,

……………………………………………

$\frac{1}{(1+r)^{n}}$ = present value of 1*l*. at the end of the v periods,

therefore, $\frac{1}{(1+r)^{v}} + \frac{1}{(1+r)^{2v}} + \frac{1}{(1+r)^{3v}} + \ldots\ldots \frac{1}{(1+r)^{n}} = \frac{1-(1+r)^{-n}}{(1+r)^{v}-1}$ = present value of 1*l*. payable every v years, during n years …………………………………… (b)

Art. 14.—When n is infinite, the last expression becomes $\frac{1}{(1+r)^{v}-1}$ = present value of a perpetuity of all such fines. For in that case $(1+r)^{-n}$ would become infinitely small.

Art. 15.—When $v = 1$, the expression (b) will become

$\frac{1-(1+r)^{-n}}{r}$ = present value of 1*l.* per annum for *n* years, and

$a \times \frac{1-(1+r)^{-n}}{r}$ = present value of an annuity *a* for *n* years.

Art. 16.—When *n* is infinite, $\frac{1}{(1+r)^n}$ will be infinitely small, and may, therefore, be neglected, the last expression in that case would become $\frac{a}{r}$ = present value of an annuity *a*, payable yearly for ever.

Art. 17.—If the interest be payable *m* equal times in a year, equation (*b*) will become $\frac{1-\left(1+\frac{r}{m}\right)^{-mn}}{\left(1+\frac{r}{m}\right)^{mv}-1}$ = present value of 1*l.* payable every *v* years, when the interest is payable *m* equal times in a year.

Art. 18.—If *v* = 1, and *m*, as in Art. 17, equation (*b*) will become $\frac{1-\left(1+\frac{r}{m}\right)^{-mn}}{\left(1+\frac{r}{m}\right)^{m}-1}$ = present value of 1*l.* per annum for *n* years, the interest payable *m* equal times in a year.

Art. 19.—If an annuity of 1*l.* per annum be payable *u* times in a year, then will the expression in Art. 17 become $\frac{1}{u} \times$

$$\frac{1-\left(1+\frac{r}{m}\right)^{-mn}}{\left(1+\frac{r}{m}\right)^{\frac{m}{u}}-1}$$ = present value of an annuity of 1*l.* per annum, payable *u* equal times in a year, when the interest is payable *m* equal times. (See Art. 12.)

Art. 20.—When *u* = *m*, the last expression will become

$$\frac{1-\left(1+\frac{r}{m}\right)^{-mn}}{r}$$ = present value of an annuity of 1*l.* per annum, when both annuity and interest are payable *m* equal times in a year.

Art. 21.—When *n* is infinite, and *u* = *m*, $\frac{1}{\left(1+\frac{r}{m}\right)^{mn}}$ in the last article will be infinitely small, in that case $\frac{1}{r}$ = present value of 1*l.* per annum, when both annuity and interest are payable *m* equal times in a year for ever. (See Art. 16.)

Art. 22.—If in Art. 18, *n* be infinite, then will $\frac{1}{\left(1+\frac{r}{m}\right)^{m}-1}$

= present value of the perpetuity of 1*l*. per annum, the interest payable *m* times each year.

Art. 23.—If in Art. 19, *n* be infinite, then will $\frac{1}{n} \times \frac{1}{\left(1+\frac{r}{m}\right)^{\frac{m}{n}}-1}$ = present value of the perpetuity of 1*l*. per annum, when the annuity is payable *n* times in a year, and the interest *m* times.

For a fuller account on this subject, and for a variety of interesting and useful examples, see Mr. Francis Baily's "Doctrine of Interest and Annuities."

From the preceding articles, the state of the population of a country under given circumstances may easily be determined.

If in any place where there is no migration, and the increase of population observe the following law, the amount of the whole population at any given time may be determined as follows: Let P represent the population of a country at any given period; $\frac{P}{a} = B$ = number of births, and $\frac{P}{b} = D$ = number of deaths in a year, then will $\frac{P}{a} - \frac{P}{b} = \frac{b-a}{ab} . P = B - D$ = increase of population in the first year; from whence $\frac{b-a}{ab} = \frac{B-D}{P} = e$. Now if in Art. 1, for principal, we write population, for *r* we write *e*, and for *s* we put A, we shall have $P(1+e)^n = A$ = population at the end of *n* years; therefore,

$$\text{I. } (1+e)^n . P = A,$$

$$\text{II. } \frac{A}{(1+e)^n} = P,$$

$$\text{III. } \left(\frac{A}{P}\right)^{\frac{1}{n}} - 1 = e,$$

$$\text{IV. } \frac{l.A - l.P}{l.(1+e)} = n.$$

If *m* P = A, then will $(1+e)^n = m$, from whence $n = \frac{l.m}{l.(1+e)}$ = a period in which the population would be increased *m* times.

If the population decrease, we shall have $\frac{P}{b} - \frac{P}{a} = \frac{a-b}{ab} . P = D - B$ = decrease of population in the first year, from whence $\frac{a-b}{ab} = \frac{D-B}{P} = e$. By substituting in Art. 5, we get $(1-e)^n$. $P - A$ = decrease of population in *n* years; therefore,

$$\text{V. } (1 - e^1)^n \,.\, \text{P} = \text{A}^1,$$

$$\text{VI. } \frac{\text{A}}{(1-e^1)^n} = \text{P},$$

$$\text{VII. } e^1 = 1 - \left(\frac{\text{A}^1}{\text{P}}\right)^{\frac{1}{n}},$$

$$\text{VIII. } n = \frac{l\,.\,\text{A}^1 - l\,.\,\text{P}}{l\,.\,(1 - e^1)}.$$

If $\frac{\text{P}}{m} = \text{A}^1$, then will $(1 - e^1)^n = \frac{1}{m}$, from whence $n = l\,.\,\frac{1}{m} - l\,.\,(1-e^1) = -\,[l\,.\,m + l\,.\,(1 - e^1)]$ = period in which the population would be reduced $\frac{1}{m}$th part. If the population be increasing, we shall have by substituting in Art. 3, $\frac{\text{P}}{(1+e)^n}$ = population n years since; and if decreasing, we have by Art. 5, $\frac{\text{P}}{(1+e^1)^n}$ = population n years since. (See Mr. Milne's Annuities, vol. i, p. 103.)

Article X.

Analyses of Books.

Philosophical Transactions of the Royal Society of London, for 1822. *Part II.*

We hasten, by our promptitude in the analysis of this part, which has just been published, to compensate for our tardiness in reviewing the former one. It contains the following papers:

XIX. *Experiments and Observations on the Development of Magnetical Properties in Steel and Iron by Percussion.* By William Scoresby, Jun. Esq. (Communicated by Sir Humphry Davy, Bart. PRS.)

"Dr. Gilbert, so early as the year 1600, discovered that iron became sensibly magnetic on being hammered and drawn out while lying in a north and south direction;" but Mr. Scoresby cannot discover "that any magnetical effect by hammering has been produced beyond that of occasioning a deviation in the compass needle, or of giving to floating bars or needles the power of conforming their position to that of the magnetic meridian."

Mr. S. having already "succeeded in determining, in a great measure, the principal laws by which the development and destruction of magnetism in iron by percussion, scowering, filing, bending, &c. are governed," and which have been published in the Edin. Phil. Trans. for 1821, confines himself, in the present communication, "to the application of these laws to practical magnetism; and particularly to the construction of magnets, without the use of any magnetized substance."

"In examining the magnetical effect of percussion on different kinds of iron and steel, two tests were employed; the weight of iron that the body would lift, and the quantity of deviation that it would produce on a magnetic needle when presented to it in a certain position, and at a certain distance. For the first test, common iron nails of different sizes were made use of: they were of the weights of 2, 4, 6½, 14, 24, 37, 45, 88, 130, and 188 grs. For the purpose of securing a good and uniform contact with the magnetized bar, the oxide on the ends of the nails was removed by means of a fine file, and the extremities were then polished by rubbing them on a Turkey stone. The second test I employed consisted of a board two feet in length, with a longitudinal line down the middle divided into inches, and a sensible pocket compass. To guard against the effects of the magnetism of position, the central line of the board was placed exactly in an east and west direction by the compass; and as the board was laid horizontally on a table, this line was known to be in the plane of the magnetic equator, and consequently in a situation in which small bars of iron are not affected by the magnetism of position. In applying this simple apparatus as a measure of magnetism; the bar, whose magnetism was to be examined or compared, was laid along the central line of the board, with its north pole always nearest the compass. The compass was placed with its centre at the commencement of the scale, so that its needle was exactly at right angles to the direction of the bar; and before the deviation took place, its poles were equidistant from the bar. The distance was estimated by the scale on the board, and always represented the space between the north end, or nearest end of the bar, and the centre of the compass. Three hammers were also employed: No. 1, of 22 ounces; No. 2, of 12 ounces; and No. 3, of 2½ ounces weight.

With this apparatus, a number of experiments were performed, several tables of which are given: and their general results are stated as follows:

1. A cylindrical bar of *soft steel*, 6½ inches long, and weighing 592 grains, lifted, after repeated hammering on pewter and stone, 6½ grains; but could not be made to lift a nail of 11 grains.

2. The same bar hammered vertically upon a parlour poker, also held erect, after 22 blows, lifted with the lower end, which was a north pole, 88 grains; and on using a larger hammer, received a considerable increase of power, producing a deviation of the compass, three inches distant, of 34 degrees; further hammering, it was found, rather diminished than increas-

ed the effect. On the bar being inverted, so that the north pole was upward, the magnetism was very nearly destroyed by a single blow; while two blows changed the poles. Hammering the end of the bar in the plane of the magnetic equator also destroyed the polarity; but the effect was not fully produced until many blows had been struck.

"When the poker had been previously hammered in a vertical position, an increase of magnetic effect on the bar was obtained, a single blow being now sufficient to enable the bar to lift about 20 grains; and when the end was hammered into a kind of cup, so as to be easily bruised, the bar was by one blow rendered capable of lifting between 30 and 40 grains. After 10 blows, the highest effect obtained in all the experiments was produced, the same bar readily lifting a nail of 188 grains, being nearly one-third of its own weight!

"The magnetism by percussion was found by subsequent experiments to be augmented when the length of the bars was increased; thus a quarter-inch cylindrical bar of steel five inches long, after receiving 20 smart blows, produced a deflection of the needle, at the distance of three inches, of 13°, and lifted 6½ grains. Another piece of the same bar 7¾ inches long, after similar treatment, produced a deviation of 24°, and lifted 45 grains; and a third bar of the same kind 12 inches long, after 20 similar blows, occasioned a deviation of the compass of 33°, and easily lifted 88 grains. The shortest bar, it was observed, received the full effect by the first two blows; but the others continued to increase in energy as the percussion was continued. These bars did not receive a power equal to that first used; the cause was probably their greater hardness.

"3. A *strong magnet* properly tempered was injured in whatever position it was hammered, but most rapidly when the north pole was upward. After no further diminution of its magnetism could be produced with the south end upward, a quick loss of power was effected by hammering it with the north pole upward. But after the magnetism had been reduced to a certain extent by hammering in both positions, the power became nearly stationary; so that on striking it in any position with the same hammer, very little change of intensity occurred."

Besides these results, the author mentions the effect of percussion on soft steel magnets, on soft iron not magnetized, and on cast iron. *One of the first* capable of lifting upwards of 1000 grains, when placed vertically upon the poker with its north pole upward, had its magnetism destroyed by five blows. A bar of *soft iron* of the same size and form as the steel bar first described, and weighing about 600 drains, was hammered for a considerable time while held vertically upon the poker. The greatest effect which he could produce with the large hammer was a deflection of the compass needle, at the distance of three inches, *of 13 degrees.* In this state it lifted a nail of 6½ grains, but refused one of 11 grains weight. A similar bar of *cast-iron* became capable of lifting 37 grains; and after it had acquired this power, its magnetism was nearly destroyed by five blows with the north pole upward.

The strong magnetising effect of percussion on soft steel induced Mr. Scoresby to apply the property to the formation of magnets. In accomplishing this object he took particular care that no magnetic substance should be used in the process, which he describes in the following terms:

"I procured two bars of soft steel, 30 inches long, and an inch broad; also six other flat bars of soft steel 8 inches long, and half an inch broad, and a large bar of soft iron. The large steel and iron bars were not, however, absolutely necessary, as common pokers answer the purpose very well; but I was desirous to accelerate the process by the use of substances capable of aiding the developement of the magnetical properties in steel. The large iron bar was first hammered in a vertical position. It was then laid on the ground with its acquired south pole towards the south; and upon this end of it, the large steel bars were rested while they were hammered; they were also hammered upon each other. On the summit of one of the large steel bars, each of the small bars held also vertically was hammered in succession, and in a few minutes they had all acquired considerable lifting powers. Two of the smaller bars connected by two short pieces of soft iron in the form of a parallelogram, were now rubbed with the other four bars in the manner of Canton. These were then changed for two others; and these again for the last two. After treating each pair of bars in this way for a number of times, and changing them whenever the manipulations had been continued for about a minute, the whole of the bars were at length found to be magnetized to saturation, each pair readily lifting above eight ounces!"

XX. *On the Alloys of Steel.* By J. Stodart, Esq. FRS. and Mr. M. Faraday, Chemical Assistant in the Royal Institution. (Communicated by J. Stodart, Esq. FRS.)

We purpose to give this most important paper entire in an early number of the *Annals.*

XXI. *Some Observations on the Buffy Coat of the Blood, &c.* By John Davy, MD. FRS.

This communication consists of observations on the three following subjects; which, though important to the medical philosopher, are devoid of general interest; viz. the cause of the buffy coat which appears on blood drawn from persons labouring under inflammatory disease; the fallacy of a prevalent opinion that the age of those morbid adhesions connecting serous membranes, which are so often met with in dissection, may be guessed at by their strength; and the effusions of serum found after death in the cavities of serous membranes. The author thinks, in con tradiction to the belief of many, that the latter do not take place after the cessation of vital action.

XXII. *On the Mechanism of the Spine.* By Henry Earle, Esq. FRS. Surgeon to the Foundling, and Assistant-Surgeon to St. Bartholomew's Hospital.

Mr. Earle's account of the exquisite mechanism of the spine and spinal canal in birds, and his illustration from it of the physiology and pathology of the human spine, can scarcely be abridged; and the first, unaided by the plate, would be difficultly intelligible.

XXIII. *Of the Nerves which associate the Muscles of the Chest in the Actions of Breathing, Speaking, and Expression; being a Continuation of the Paper on the Structure and Functions of the Nerves.* By Charles Bell, Esq. (Communicated by Sir Humphry Davy, Bart. LLD. PRS.)

For the anatomical and physiological details here given, and their applications to pathology, we must refer our readers to the memoir itself.

Mr. Bell informs us in the commencement, "that already practical benefits have arisen from the former paper; that the views presented there, as connected with general science, being carried into practice, have enabled the physician to make more accurate distinctions of disease, and the surgeon in removing deformity, to avoid producing distortion."

XXIV. *Experiments and Observations on the Newry Pitchstone, and its Products, and on the Formation of Pumice.* By the Right Hon. George Knox, FRS.

We are informed in the commencement of this paper, that the locality of this mineral, and the singularity of its external characters, having excited the curiosity of the author, he took advantage of the facilities afforded by the liberality of the Royal Society of Dublin to make an analysis of it in their laboratory; and after making some observations on the varying characters of the Newry pitchstone, and mentioning that Dr. Fitton seems to have overlooked two striking characters of it, the *smell* and strong oily taste, he gives from the Transactions of the Geological Society that gentleman's description of its site and characters.

Mr. Knox adds some further particulars to Dr. Fitton's geological statement, and a more particular account of the characters of the stone itself. He observes, that "although the peculiar character of this variety of pitchstone is its smell, yet, I believe, it differs from all others, including those from Arran, in the *degree* in which it is disposed to divide into thin laminæ; its proneness to disintegrate, and the regularity of its rhomboidal fragments."

A piece of the compact specimen lost 7·75 per cent. by ignition for half an hour, was changed in colour to a pitch-brown, retaining its lustre; and, without actually falling in pieces, opened and thin slaty fragments: 100 grains of the same in fine powder were exposed to a white heat in a platina crucible, and were converted into a very pale leek-green glass, losing 10 per cent.: 220 grs. coarsely powdered being ignited in a coated glass retort yielded 16 grs. or 7·2727 per cent. of a colourless fluid, having a slightly bituminous smell. "The stone finely powdered and projected on melted nitre scintillated a little."

Mr. Knox next proceeded to ascertain the constituents of the mineral, following the method of Klaproth in his analysis of the pitchstone from Meissen, which analysis he details. In thus examining the Newry pitchstone, he obtained neither manganese nor magnesia.

The muriatic solution divested of silica evaporating with considerable rapidity, a black powder separated from it, which was insoluble in acids, and burned away at a red-heat; it was at first suspected that some carbon had accidentally got into the liquor, but it was proved to belong to the stone by repetition of the experiments and by other circumstances. It was ascertained by the process with nitrate of barytes that the mineral contained soda, which was unmixed with potash or lithia.

Mr. Knox then proceeds to describe his final analysis, in which he used the *slaty-compact* variety of the pitchstone, his specimen of the compact one having been exhausted. He obtained the silica in the usual method by fusion with soda, precipitated the alumina and iron by ammonia, and separated them by boiling in caustic potash; obtained the lime by precipitating the solution freed from alumina and iron with carbonate of soda, and precipitated the alkaline solution of alumina by muriate of ammonia.

We have now arrived at that part of the analysis in which the author endeavoured to ascertain the proportion of alkali in the stone; and this we cannot but deem defective. Our chemical readers will be surprised to learn, that rejecting the process with nitrate of barytes as tedious and liable to loss, that by boracic acid as difficult, and also the new process by lead, Mr. Knox, for the purpose of extracting the soda, boiled 100 grains of the pulverized stone in dilute nitric acid, taking up by water the soluble part when the fluid had been driven off, and replacing the acid; this process being "repeated until the acid seemed to have no further effect." By this method, 7·75 grs. of nitrate of soda were obtained, giving 2·857 for soda.

We apprehend that we shall be supported by the testimony of all persons who are versed in analytical chemistry, in affirming that it is impossible to extract from a siliceous compound the whole of the alkali which it contains by this mode of operating. As to the result by muriatic acid, which the author adduces as confirmatory of the above, his statement of it is incorrect; he obtained five grains of chloride of sodium, which, he says, "make of dry soda, or oxide of sodium, 1·98198, being in the proportion 55·5 to 22; but 1·98198 of dry soda produce 2·8704 of hydrate of soda, the state in which it is probable the alkali exists in the stone."

Now the nitrate of soda obtained in the first operation being an anhydrous salt, the quantity of alkali indicated by it is o dry soda, mere oxide of sodium, and not of the hydrate; so that in reality the results of the two processes do not agree.

During the process with muriatic acid, a yellow substance having a bituminous smell, was condensed on the inside of the silver cover of a crucible; and the alcohol which had been employed to separate the chloride of calcium from that of sodium, deposited on evaporation a dark oily substance, which had "an empyreumatic smell, was insoluble in ether, but dissolved in spirits of turpentine, and inflamed with difficulty with a thick smoke, and pungent odour. Naptha dissolved it only in part, and changed the colour to grass-green."

Mr. Knox next endeavoured to obtain the bituminous matter of the stone in a state of purity, and to ascertain its quantity.

In the first experiment, 480 grains of the dark leek-green slaty variety were strongly heated in an iron retort, to which was attached a bent gun-barrel, with other necessary apparatus. A quantity of gas came over, and when the retort had acquired a red-heat, some water; the heat being urged further a slightly coloured oily liquid appeared. The gas consisted of carbonic acid; "of hydrogen, which was judged of by its inflammation;" and of carburetted hydrogen, which was tested by explosion with oxygen gas. The receiver had gained 7·81 per cent. The oily fluid had the smell of tobacco, and burned with a similar flame to naptha. The water was neither acid nor alkaline.

In another experiment with a glass retort, 2·83 per cent. of pure bitumen were obtained: in another, 100 grains of the slaty compact variety lost by ignition 8·0 per cent, and upon fusion into glass 0·5 more: 480 grains of the same were distilled after the water was expelled by ignition: bitumen came over, and after the receiver was removed, some more dropped from the retort: the latter had the same smell of tobacco as the former products; that in the receiver smelled more of naptha, and was volatilizable by the heat of the hand.

One hundred grains in mass of the Meissen pitchstone being ignited in a platina crucible, opened in the same manner as the compact variety from Newry; on fusion into enamel, it lost eight per cent: 400 grains being distilled after ignition yielded a small quantity of bitumen more volatile than any of the former, and having the smell of naptha.

One hundred grains of Arran pitchstone being fused into glass, lost five per cent.; 400 grains yielded on distillation some water, with indications of bitumen.

Respecting the latter substance in the Newry mineral, the following remarks are made: "It seems to consist of two inflamvolatile substances, the one much more volatile than the other, both inseparable from the stone, except at a heat approaching, if not entirely amounting to, whiteness. I imagine that it is in combination with the iron, as it seemed in general to accompany the solutions of that substance, and to modify the colour and magnetic properties of the metal.....If it should be found to be a new substance, I propose to call it *newrine*. I should not be surprised, however, judging from the smell, and its being inseparable from water by evaporation, if it were found to contain alcohol in combination with naptha."

Result of the analysis of the pitchstone of Newry:

Silica	72·800
Alumine	11·500
Lime	1·120
Protoxide of iron	3·036
Soda	2·857
Water and bitumen	8·500
	99·813

The mass which remained in the retorts after the distillation of this mineral was *pumice*, having the colour, levity, and magnetic properties of the natural substance, and deceiving artists to whom it was presented as such. "It appears to be a condition," says the experimenter, "in converting a stone into pumice, that it should contain a volatile substance, which can only be removed by the same degree of heat which is at the same time necessary for producing that sort of semi-vitrification in the mass which renders it coherent, hard, and porous." Some greenstone, which had lost 1·25 per cent. by ignition, was treated as the pitchstone had been, and became converted, partly into a vesicular glass, and partly into pumice.

XXV. *Observations on the Changes the Egg undergoes during Incubation in the common Fowl; illustrated by microscopical Drawings.* By Sir Everard Home, Bart. VPRS.

In this communication, the progress of the formation of the chick is traced, "step by step, from the first appearance of the molecule found on the yolk before it leaves the ovarium to the complete evolution of all its parts, and its leaving the shell." The details are illustrated by a series of microscopical delineations by Mr. Bauer, and cannot usefully be given without them.

Sir Everard proceeds to illustrate from the results of the investigation, the processes by which the human fœtus and that of quadrupeds are formed, some circumstances being common to them, and those employed in the bird.

XXVI. *Some Observations on Corrosive Sublimate.* By John Davy, M.D. FRS.

Dr. Davy commences this paper by observing, "I am not aware that the operation of light on corrosive sublimate has yet been minutely considered. It is known that the *liquor hydrargyri oxymuriatis* of the London Pharmacopœia is decomposed by light; it has been stated that the compound itself, when exposed to light, undergoes the same change; and it has been recommended in consequence, to keep it in black bottles."

With a view to acquire some precise information on this subject, the following experiments were instituted by Dr. D. Some corrosive sublimate in fine powder was exposed to sunshine for 14 days in a sealed glass tube; no change was then produced. A solution in distilled water being exposed to sunshine for the same period, calomel and free muriatic acid resulted. "Some *liquor hydrargyri oxymuriatis* and solutions of corrosive sublimate in rectified spirit and in ether were exposed to sunshine for the same time." In the former, calomel was produced; while in the two latter, no change took place. Oil of turpentine being poured on the sublimate, and exposed to sunshine, had its fluidity slightly impaired; but the sublimate was unaltered. "To a saturated aqueous solution of corrosive sublimate, a few drops of muriatic acid were added; and to another saturated solution, a small quantity of muriate of ammonia. No change was produced in these solutions by the action of light during exposure for three weeks."

"From these experiments it may be deduced," continues the author, "that light alone has not the power of decomposing corrosive sublimate, and that it does not produce the effect, excepting when aided by affinities of a complicated nature."

In confirmation of this conclusion, some other experiments are related. It was found that 37 grains of distilled water were required to dissolve 2 grains of corrosive sublimate at the temperature of 57° Fahrenheit; and that its degree of solubility increases greatly with the temperature. Alcohol, of specific gravity ·816, at 60°, dissolves half its weight of the same substance; the specific gravity of the solution 1·08. Twenty grains of sulphuric ether, of specific gravity ·745, took up 7 grains; the specific gravity of the solution being likewise 1·08. The solvent power of ether does not appear to be increased by elevation of temperature, or diminished by its reduction; the boiling point of the solution and of pure ether seems to be the same; and in the act of ebullition, the solution appears to be decomposed.

When a mixture of corrosive sublimate and oil of turpentine is gently heated, mutual decomposition takes place. "The results appear to be modified by the proportions of the two substances. When the quantity of corrosive sublimate is large, the whole of the oil appears to be completely decomposed, and the products are, liquid muriatic acid, calomel, and charcoal. When the oil is in excess, the part that escapes decomposition passes over impregnated with muriatic acid; and, judging from its *smell*, appears to contain a minute quantity of artificial camphor."

Dr. Davy believes "that changes very similar take place when corrosive sublimate is heated with other oils, both volatile and fixed;" and states the experiments on which that belief is founded.

He then says, "In a paper published in the Philosophical Transactions for 1812, I have noticed the affinity of muriatic acid for corrosive sublimate. This solution may be considered as composed of 11 proportions of water, 1 muriatic acid,* and 4 salt. In the act of forming, heat is evolved. At 74° this solution is of specific gravity 2·412. When its temperature is lowered a few degrees, it suddenly becomes solid, and forms a mass of delicate needle crystals, which rapidly melt, when the containing vessel is held in the warm hand."

Dr. Davy next adverts to the common statement in systematic works, that corrosive sublimate is soluble in the sulphuric and nitric acids, as well as in the muriatic acid. He shows from experiment that this is not the case; and then proceeds to relate in the following terms some experiments which tend to corroborate an opinion long ago entertained, that muriate of ammonia and corrosive sublimate are capable of uniting and of forming a double salt.

"In the dry way, there appears to be an affinity exercised between corrosive sublimate and muriate of ammonia. A mixture of the two, in the proportions of 34 of the former and 6·75 by weight of the latter, heated, forms a compound more fusible and less volatile than either ingredient separate; it may be kept liquid without volatilising by the gentle heat of a spirit lamp; on cooling, it exhibits a very light-grey translucent mass of a faint pearly lustre; strongly heated, it sublimes, and appears to be partially decomposed, as traces of calomel and free muriatic acid are found mixed with the sublimate. This compound, formed of one proportion of each ingredient, has more the character of a chemical compound, than any other mixture of the two ingredients that I have tried."

"In the moist way, the affinities of corrosive sublimate and muriate of ammonia are better marked, and some of the combinations of the two have tolerably well-defined qualities. The following have the best claim to be considered distinct combinations of any which I have yet made:

No.	Water.	Muriate of ammonia.	Corrosive sublimate.
1	9·00 grs.	6·75	34·00
2	9·00	3·37	17·00
3	9·00	3·37	8·50
4	9·00	10·12	25·50

No. 1 is liquid at 140°; on cooling, it forms a solid mass of needle crystals. No. 2 is liquid at 85°, and solid at 55°. In the liquid form, at the temperature just mentioned, it is of specific gravity 1·98. No. 3 is liquid at 55, and of specific gravity 1·58. No. 4 is liquid at about 105°; on cooling slowly to 60°, it deposits some crystals which are four-sided prisms, composed of facets alternately broad and narrow."

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* New System of Chem. Phil. by John Dalton, vol. ii. p. 295.

Some circumstances are here mentioned which prove that corrosive sublimate and muriate of ammonia have a strong affinity for each other, and among them the results of an experiment, from which "it would appear that corrosive sublimate is about 17 times more soluble in a saturated solution of muriate of ammonia than in water, and not 30 times, as is stated by some authors."

"The results of these experiments," continues Dr. D. "led me to make trial of some other muriates, as of baryta, magnesia, potash, and soda." The trials with these substances are then described in detail, and it is inquired respecting their results toward the conclusion of the paper, "May not the compounds of corrosive sublimate and common salt, muriate of magnesia and baryta, respectively, be considered as constituted of one proportion of each ingredient? The definite nature of the compounds with muriate of ammonia and potash are, perhaps, more questionable."

It is next remarked, that all these compounds exhibit the properties of the most active constituent, and the paper concludes as follows: "It would appear, from the preceding experiments, that those menstrua which have a strong affinity for corrosive sublimate, prevent its decomposition when exposed to light, as the muriates, alcohol, and ether; and, on the contrary, that those solvents which exercise a weak affinity on it, and have a stronger affinity for muriatic acid, as water, and exceedingly dilute alcohol, aid the decomposing power of light. The practical application to be deduced, relative to the formula for the *liquor hydrargyri oxymuriatis*, is obvious, and does not require to be pointed out."

XXVII. *On the State of Water and Aëriform Matter in Cavities found in certain Crystals.* By Sir Humphry Davy, Bart. PRS.

This novel investigation of a contested subject in geology we purpose to insert at large in the *Annals*.

XXVIII. *Some Experiments on the Changes which take Place in the fixed Principles of the Egg during Incubation.* By William Prout, MD. FRS.

An abstract of this very interesting communication will shortly appear in the *Annals*.

XXIX. *On the Placenta.* By Sir Everard Home, Bart. VPRS.

This paper relates, primarily, to certain operations of utero-gestation; to the means employed by nature to prevent any two different genera from breeding together; to the period of utero-gestation; and to the direct cause of parturition. It concludes with a specimen of a new mode of classing animals according to the difference in structure of the placenta; or where this is wanting, of the chorion; and is illustrated with seven plates.

XXX. *Of the Geographical Situation of the Three Presidencies, Calcutta, Madras, and Bombay, in the East Indies;* and

XXXI. *Of the Difference of Longitudes found by Chronometer, and by correspondent Eclipses of the Satellites of Jupiter; with some Supplementary Information relative to Madras, Bombay, and Canton; as also the Latitude and Longitude of Point de Galle, and the Friar's Hood.* By J. Goldingham, Esq. FRS.

The following are the results of the observations described or enumerated in these papers.

	Long. E. of Greenwich.			Lat. N.		
Madras, the Observatory	80°	17′	21″	13°	4′	9·1″
Madras, Fort St. George Church Steeple	80	19	42	13	4	45
Madras, the Fort Flag Staff	80	19	44	13	4	47
Calcutta, Fort William	88	23	39		—	
Bombay, the Church	72	54	43	18	56	7
Bombay, the Lighthouse	72	53	36	18	54	25
Masulipatam Flag Staff	81	12	24		—	
Point de Galle Flag Staff	80	17	2	6	0	50
The Friar's Hood	81	36	3¼	7	29	35
Canton	113	18	23		—	

On the coast about Madras, the tide seldom rises more than three feet; and it is high water in the Syzigies at $9^h\ 25^m$; the variation of the compass towards the end of 1792 on the coast, about a degree to the northward of Madras, was 1° 3′ E.

At Bombay, the time of high water in the Syzigies at the Dock Head was $11^h\ 32^m$; the greatest rise of the tide 18 feet; medium rise of the springs 15¼ feet; variation of the compass in the beginning of 1791, 42′ 59″, or 43′ W.

XXXII. *Observations on the Genus Planaria.* By J. R. Johnson, MD. FRS.

This paper relates to the natural history of four species of this genus: *P. cornuta, P. torva, P. brunnea,* and *P. lactea;* of which Dr. Johnson describes characters and habits, but we can only notice their manner of feeding, and mode of perpetuating their species.

There are, in these curious animals, two ventral apertures, the upper of which gives passage to a long flexible tube, and the lower conducts to the ovarium; the tube they frequently project, and it nearly equals the body in length.

A variety of aquatic insects, worms, &c. being presented to some *planariæ,* one of them, after the lapse of a few minutes, fastened upon a worm, immediately projecting and affixing this tube; the worm being in this way closely retained, other *plana-*

riæ came forward, and completely overpowered it. They seldom attacked the worm openly, seemingly aware of the difficulty of thus overcoming it, but seized upon it, as it were, by stealth, gliding gently underneath it, and then projecting and affixing this organ, keeping a firm hold until they had concluded their repast. Dr. Johnson at first imagined the sole use of this organ to be that of effectually securing their prey; but he observed that while they kept it firmly affixed, they moved their heads freely from side to side; and he found from a number of experiments, that when the *planariæ* were perfect animals, they constantly received their food by this organ, and not by the mouth. In the event, however, of their being naturally or artificially divided, or of their losing this tube, which was not unfrequently the case, they took their usual sustenance by the mouth. Thus an animal furnished with a proper mouth, receives its food by another organ, that organ being placed as near to the tail as to the head.

The *P. lactea* and *P. brunnea* "are oviparous, producing eggs, within a membranous capsule, each egg containing (at least those of the *P. lactea*), from three to eight young." But these animals have another mode of perpetuating the species, which "does not appear yet to have been noticed;" this is "by a *natural* division of the body into two portions, the head part reproducing a tail, and the tail a head, in about 14 or more days, depending upon the state of the atmosphere." Preparatory to this division, the posterior portion of the body first widens, and afterwards the animal has the segmented appearance of an insect. "On the third day, the separation of the head from the tail usually takes place. When undergoing this division, they remain for the most part stationary, keeping the head firmly affixed, twisting round the tail from time to time with a view of lessening its adhesion, and thus more readily effecting its disunion. Almost immediately after the head is liberated, it is seen to move with all the freedom of the unmutilated, perfect animal. The tail generally remains attached, and only occasionally shifts its situation; but if touched, it moves with nearly the same quickness as the anterior extremity, preserving a uniform gliding motion." The reproductive power of these animals when artificially divided is alluded to by Müller, Shaw, and others: Dr. Johnson describes some curious experiments on the subject.

XXXIII. *Some Experiments and Researches on the Saline Contents of Sea-water, undertaken with a View to correct and improve its Chemical Analysis.* By Alexander Marcet, MD. FRS. Honorary Professor of Chemistry at Geneva.

XXXIV. *On the Ultimate Analysis of Vegetable and Animal Substances.* By Andrew Ure, MD. FRS.

Dr. Marcet's paper, which displays the elegant precision that distinguished all researches of its lamented author, shall be given in a future number of the *Annals;* and of Dr. Ure's communication we shall take an opportunity of inserting an abstract.

Article XI.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Death of Count Berthollet.*

The decease of this distinguished member of the French school of chemistry has recently been announced; it took place on the 6th of November, in the 74th year of his age. He was born at Talloine, in Savoy, and was originally of the medical profession: with the results of his labours in chemical science, our readers must be well acquainted; they are numerous, and of the highest importance.

II. *Green Ore of Uranium from Cornwall.*

In preparing some oxide of uranium from this substance, I have found that it contains phosphoric acid, and not merely the oxides of uranium and copper combined with water, as has been stated. In the next number of the *Annals*, I purpose to give an analysis of this ore, accompanied by an account of some experiments on the oxide of uranium.—*Edit.*

III. *Prof. Berzelius on the Sulphurets.*

I had intended to have given in the present number of the *Annals* an abstract of the remaining portion of Berzelius's memoir on the sulphurets; but having concluded that part of it which includes the alkaline sulphurets, I beg to refer the reader to the Annales de Chimie et de Physique, tome xx. p. 128, for the remainder, beginning "Des Combinaisons Sulfures Metalliques avec les Alcalis." This paper is replete with the symbols peculiar to Berzelius, and they are so generally unaccompanied by any explanation, that it is extremely difficult to reduce them to an intelligible form; for example, in about 20 lines there occur eight symbols of the following kind: $As\,S^3 + 6\,Ag\,S^2$.—*Edit.*

IV. *Action of Magnesia on Salep.*

M. Brandes, of Hoxter, has made some experiments on a compound which is formed when calcined magnesia is put into a solution of salep. When 20 grains of salep were dissolved in four ounces of distilled water, and 30 grains of pure magnesia were added, the whole became, after some hours solid, and jelly-like; and even after a month, it had not assumed the least putrid smell. Carbonate of magnesia had the same effect, but in a smaller degree. Neither the white of eggs, nor tragacanth gum, a weak solution of isinglass, nor of starch, assumed on addition of magnesia a similar jelly-like appearance. The mucilage of quince seeds deposited the magnesia with a granular appearance, and the solution seemed to have become more fluid. Neither lime nor white bole produced a similar effect upon the solution of salep. The jelly is insoluble in water, fat oils, oil of turpentine, alcohol, or a solution of caustic potash. Acids, principally sulphuric acid, dissolve it partly; the remainder is more bulky and is opalescent.

V. *Ancient Aërolite.*

A Danish journal mentions a fact (taken from the Speculum Regale, a book written in the thirteenth century under the reign of the Danish king Snorro, and by some supposed to be written by the king himself), of which it would be interesting to ascertain whether any trace remains yet in Iceland. In this book, is told that in the church of Kloena, in Iceland, an anchor is kept, which had fallen from the air; and, adds the Danish journalist, it is probable that it was an aërolite in form resembling an anchor, or that an anchor had been made of this meteoric iron.

VI. *Mathematical Laws of Electro-Magnetism.*

Mr. Barlow, who has so successfully reduced the laws of induced magnetism to mathematical principles, has been equally fortunate in rendering electro-magnetism a matter of computation. The battery he employed was on the principle of Dr. Hare's Calorimotor, and the experiments were made by means of a rectangle of stout brass wire, each side of which was four feet. One side of this was open, so as to make the connexion with the battery, and the other vertical side was passed through the centre of a table, divided into the points of the compass, and round which, therefore, a magnetic needle might be placed at any azimuth. The two horizontal sides of the rectangle might be slipped up and down on the vertical wires, whereby the length of the conducting part of the vertical wire might be changed at pleasure; and the distance of the compass itself from the vertical wire might also, in like manner, be varied *ad libitum*, by merely sliding to and from the centre. From his experiments with this apparatus, Mr. Barlow has drawn the following general conclusions, viz. "that every particle of the galvanic fluid in the conducting wire acts on every particle of the magnetic fluid in a magnetized needle, by a force which varies inversely as the square of the distance; but that this action is neither to attract nor to repel either pole of the magnetic particles, but a tangential force, which is reciprocal between the two fluids, and which tends to place the poles of either at right angles to those of the other, and to the right line which joins them." This theory is said to be applicable to every phenomenon that has yet been observed in this new branch of natural philosophy.—(Edin. Phil. Jour. vii. 281.)

VII. *Azotic Springs in North America.*

In the south-east corner of the town of Hosick, Rensselaer county, New York, are three springs comprised within about four or five acres of ground, from which issues an incalculable quantity of pure nitrogen gas. It seems to rise from the gravel-beds beneath the water; by pressing upon a surface of the gravel equal to five or six inches square, a quart of the gas may be collected in an inverted jar or bottle in ten seconds.—(Ibid. p. 387, from a Geological Survey of the County.)

VIII. *A new Mineral called Gibbsite.*

This substance, named after a celebrated American mineralogist, was discovered by Dr. E. Emmons, in a neglected mine of brown hæmatite in the town of Richmond, Massachusets. It occurs in irregular stalactitic masses, from one to three inches in length, and one or more in breadth, consisting of elongated tuberous branches united in a parallel direction. It is rather harder than calcareous spar, is slightly translucid, and has a specific gravity of 2·40. It does not effervesce with acids, and whitens before the blowpipe.

According to the analysis of Dr. Torrey, of New York, who was particularly careful in ascertaining that it contained neither fluoric acid nor phosphoric acid, the constituents of gibbsite are:

Alumina	64·8
Water	34·7
Loss	0·5
	100·0

(Ibid. p. 388, from the New York Med. and Phys. Jour. No. 1.)

IX. *Tungstate of Lime.*

A specimen of this substance found in America, and analyzed by Mr. Bower, gave

Tungstic acid	76·05
Lime	14·36
Silica	2·54
Oxide of iron	1·03
Oxide of manganese	0·31
	99·29
Loss	0·79
	100·00

(Silliman's Journal.)

ARTICLE XII.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

Dr. Henry is printing a New Edition of his Elements of Experimental Chemistry, with great Additions, and Alterations.

A Journal of a Horticultural Tour through some Parts of Flanders, Holland, and the North of France in the Autumn of 1817, by a Deputation of the Caledonian Horticultural Society.

JUST PUBLISHED.

Transactions of the Geological Society, Vol. I. Part I. Second Series. 4to. 1*l.* 11*s.* 6*d.*—This volume contains the following papers: On the Geology of the Southern Coast of England, from Bridport to Babbicombe Bay; by H. J. de la Beche, Esq. On the Bagshot Sand; by Henry Warburton, Esq. On a Freshwater Formation in Hordwell Cliff; by Mr. Webster. On Glen Tilt; by Dr. Mac Culloch. On the Excavation of Valleys by Diluvian Action; by the Rev. Prof. Buckland. On the Icthyosaurus and Plesiosaurus, by the Rev. W. Conybeare. Outline of the Geology of Russia; by the Hon. W. T. H. Fox Strangways. On the Geology of the Coast of France, Department de la Seine inferieure, by H. J. de la Beche, Esq. On the Valley of the Sutluj in the Himalaya Mountains; by H. J. Colebrooke, Esq. On the Geology of the North-eastern Border of Bengal; by H. J. Colebrooke, Esq.; with various other Papers and Notices; the whole illustrated by 24 Plates, Maps, and Sections, many of them coloured.

A Succinct Account of the Lime Rocks of Plymouth, being the Substance of several Communications read before the Geological Society of London: with 10 Lithographic Plates. By the Rev. Richard Hennah, Chaplain to the Garrison of Plymouth. Royal 8vo. 12*s*.

Select Dissertations on several Subjects of Medical Science. By Sir Gilbert Blane, Bart. FRS. Physician to the King, &c. &c. 8vo. 12*s*.

Lectures in which the Nature and Properties of Oxalic Acid are contrasted with those of Epsom Salts. By Robert Venables, MD. T. C. D. 2*s*. 6*d*.

An Address to Parents on the Present State of Vaccination in this Country, with an impartial Estimate of the Protection which it is calculated to afford against the Small-pox. By a Candid Observer. 8vo. 3*s*.

A Guide to the County of Wicklow: illustrated by five highly finished Engravings after the Designs of George Petrie, Esq. and a large Map of the County from an original Survey. By the Rev. G. N. Wright, AM. Royal 18mo. 7*s*.

ARTICLE XIII.

NEW PATENTS.

J. Collier, Crompton-street, Brunswick-square, for improvements upon machines for shearing cloth.—Sept. 27.

W. Goodman, Coventry, hatter, for improvements in looms.—Sept. 27.

J. Bourdieu, Lime-street, London, Esq. for improving the preparation of colours for printing wove cloths.—Sept. 27.

B. Boothby, iron works, Chesterfield, for an improved method of manufacturing cannon-shot.—Sept. 27.

J. Moxon, Liverpool, merchant, and J. Fraser, King-street, Commercial-road, engineer, for improvements in ship cabooses or hearths, and also for apparatus to be occasionally connected therewith, for evaporating and condensing water.—Sept. 27.

F. L. Fatton, New Bond-street, watchmaker, for improvements on watches or chronometers.—Sept. 27.

T. T. Benningfield, High-street, Whitechapel, tobacco-manufacturer, and J. T. Beale, Christian-street, St. George's in the East, cabinet-maker, for improvements on steam-engines.—Sept. 27.

J. Frost, Finchley, for a new method of casting or constructing foundations, piers, walls, &c.—Sept. 27.

ARTICLE XIV.

METEOROLOGICAL TABLE.

1822.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Daniell's hyg. at noon.
		Max.	Min.	Max.	Min.			
10th Mon.								
Oct. 1	E	29·87	29·81	56	45	—	—	
2	E	29·85	29·82	66	56	—	35	
3	S E	29·85	29·82	69	46	—	08	
4	E	29·82	29·80	67	53	—	05	
5	S W	29·80	29·62	67	46	—	29	
6	N W	29·62	29·59	58	46	—	06	
7	S W	29·75	29·67	61	49	—	05	
8	S W	29·79	29·72	60	49	—	14	
9	S W	29·96	29·72	65	46	—	16	
10	W	30·25	29·96	60	41	—	13	
11	S W	30·25	30·03	58	45	91		
12	S E	30·03	29·57	62	56	—	02	
13	S	29·97	29·57	60	44	—	23	
14	N W	30·08	29·99	52	30	—		
15	S W	29·99	29·48	50	44	—	40	
16	Var.	29·48	29·45	56	47	—	30	
17	N E	29·73	29·45	51	38	—	24	
18	N W	29·74	29·63	51	39	—	15	
19	S W	29·65	29·57	58	49	—	35	
20	S W	29·66	29·65	59	47	—	18	
21	S W	29·84	29·66	61	42	—	10	
22	W	29·88	29·81	55	30	—		
23	S E	29·81	29·52	57	50	—		
24	S E	29·60	29·49	63	42	78	11	
25	S E	29·63	29·61	63	42	—		
26	S E	29·67	29·63	60	48	—	13	
27	N W	29·90	29·67	55	30	—		
28	Var.	30·01	29·90	58	38	—	08	
29	S W	30·07	30·01	57	45	—	02	
30	S E	30·07	29·83	61	47	—		
31	S E	29·97	29·84	62	43	30		
		30·25	29·45	69	30	1·99	3·62	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Tenth Month.—1. Fine. 2. Rainy. 3. Cloudy and fine. 4. Foggy morning: very frequent lightning in the evening: some thunder: night stormy. 5. Cloudy. 6. Cloudy; windy. 7. Cloudy. 8. Rain. 9. Variable. 10. Fine morning: rainy night. 11. Fine. 12. Cloudy. 13. Rainy: stormy night. 14. Fine. 15. Cloudy: rainy night. 16, 17. Rainy. 18. Day fine: night rainy. 19. Rainy: a storm of thunder, lightning, and hail, between twelve and one. 20. Rainy. 21. Showery: night boisterous. 22. Fine: *Stratus* in the marshes at night. 23. Fine. 24. Day fine: evening rainy. 25. Fine. 26. Cloudy: fine. 27. Fine: *Stratus* on the marshes at night. 28. Fine. 29. Cloudy. 30. Cloudy. 31. Fine.

RESULTS.

Winds: NE, 1; E, 3; SE, 8; S, 1; SW, 10; W, 2; NW, 4; Var. 2.

Barometer:	Mean height	
	For the month	29·717 inches.
	For the lunar period, ending the 7th	29·967
	For 13 days, ending the 12th (moon north)	29·838
	For 14 days, ending the 26th (moon south)	29·687
Thermometer:	Mean height	
	For the month	51·792
	For the lunar period	54·956
	For 30 days, the sun in Libra	51·883
Evaporation		1·99 in.
Rain		3·62

Laboratory, Stratford, Eleventh Month, 21, 1822. R. HOWARD.

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